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ELEMENTARY DISCUSSION OF STRENGTH OF BEAMS UNDER TRANSVERSE LOADS.

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Written for Van Nostrand's Engineering Magazine.

III.

DOUBLE FLANGED BEAMS.

So far we have considered beams with rectangular cross sections, and beams of uniform strength deduced from these. We will now consider beams of ⌶ shape.

It is evident from the investigation already given of the condition of stress in transversely loaded beams, that those portions of the beam nearest the centre bear but a small proportion of the stress, while the contrary is the case with the outside fibres. Hence we would gain strength by moving a considerable portion of that about the neutral axis and placing it on the top and bottom.

The first form in which the idea was applied was in the T or ⊥ cast iron beam. The fact that rectangular cast-iron beam always broke by the tearing of the fibres on the side subjected to tension, suggested the idea of reinforcing that side of the beam with a flange. The result of this is, that the neutral axis still passing through the centre of gravity of the cross section, the extreme fibres subjected to compression are farther off than those subjected to tension, and consequently are strained more nearly to their full strength before fracture. This form of beam gives a large increase of strength for the same amount of iron.

It was still plain that the fibres in that part of the web about the neutral axis were but little strained as compared with the fibres on the outside, and it was proposed to leave as little material there as possible, and to place the mass of it in two flanges (⌶), one above and the other below, giving to these flanges sizes inversely proportional to the tensile and compressive strength of the material. The question then was, how much of the material should be left in the *web*, for plainly all could not be taken. The amount to be left is determined by experiment. If the web is left too thin, the beam will twist and break under the shearing force, and in some cases, from the want of stiffness in the compressed flange.

To simplify the calculations, the web is considered as bearing all the shearing stress, and no other, and the flanges as bearing all the extension and compression due to the bending moment; and these parts should be proportional accordingly with due reference to the practical difficulties that sometimes occur. The ordinary formulas for the strength of such beams are gotten by the following approximation: We first neglect the compressive and tensile forces of the

web, which are small compared with those of the flanges, and consider it as bearing only the shearing stress. Then as the depth of the flanges is generally small as compared with the depth of the beam, we consider all the fibres in each flange as strained alike, and as bearing the average stress that is brought on that flange. (Fig. 60.)

The resultant of the force on each

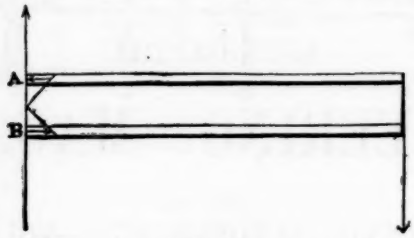


FIG. 60.

flange, then, is equal to the stress on a unit of surface (S) multiplied by the flange area (A): that is $= S A$.

The point of application of the force will be at the middle of the depth of the flanges (at O and O', Fig. 61). Fig. 61

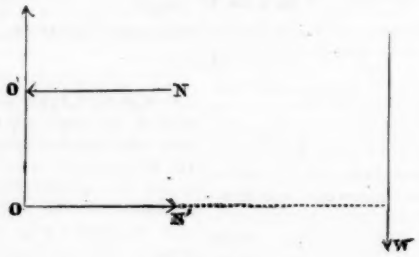


FIG. 61.

shows the forces we have to deal with in the *Case* corresponding to *Case I.* under rectangular beams.

Let $O' O = d$.

S' = stress on upper flange per unit of surface.

S'' = stress on lower flange per unit of surface.

A'' = area of lower flange.

A' = area of upper flange.

Then if we take O (Fig. 61) as a centre of moments we have:

$$-N d - N'.0. + W x = 0 \quad (\text{But } N = S' A')$$
$$\therefore S' A' d = W x \qquad (57)$$

If we take O' as the centre of moments we will get

$$S'' A'' d = W x \qquad (58)$$

The formula for shearing force is identical with that under *Case I.* of rectangular beams; that is:

$$T = W x \qquad (59)$$

If $A' = A''$, then plainly $S' = S''$ (from equations 57 and 58), or, the forces of tension and compression are equal (as in rectangular beams); but if A' and A'' are not equal, we have:

$$S' : S'' :: \frac{W x}{A' d} : \frac{W x}{A'' d} :: A'' : A'$$

That is, the unit stresses in the flanges are inversely as the areas. Now, to have the material distributed between the flanges most efficiently for strength the unit stress should be in proportion to the ultimate strength of the material against tension and compression, and hence the areas of the cross sections of the flanges should be *inversely* as the ultimate strength.

Thus, if A D (Fig. 62) be of cast-iron, which is six times as strong against compression as against tension, the unit stress in the lower flange should be made

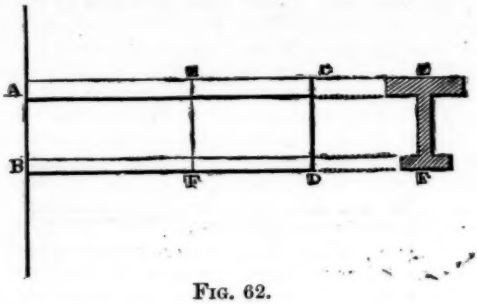


FIG. 62.

six times as great as in the upper, and to effect this the area of the lower flange should be one-sixth that of the upper.

The *Cases* under ⌶ beams are similar to those under rectangular beams.

Cass I.—Beams fixed at one end and loaded at the other.

$$S' A' d = W x, \text{ and } S'' A'' d = W x \quad (60)$$

Case II.—Beams fixed at one end and loaded uniformly.

$$S' A' d = \tfrac{1}{2} w x^2, \text{ and } S'' A'' d = \tfrac{1}{2} w x^2 \quad (61)$$

Case III.—Beams supported at both ends and loaded at some intermediate point.

$$W \cdot \frac{n}{l} \cdot x - W(x-m) = S' A' d, \text{ or, } = S'' A'' d \quad (62)$$

Case IV.—Beams supported at both ends and loaded uniformly.

$$S' A' d = \tfrac{1}{2} w x (l-x) = S'' A'' d \quad (63)$$

Case V.—A single moving load over a beam supported at both ends.

$$S' A' d = \frac{W x}{l} (l-x) = S'' A'' d \quad (64)$$

Case VI.—A distributed moving load may be considered as included in *Case IV.*

The formulae for shearing stress are identical with those in rectangular beams.

The principles of the *uniform strength of beams* may be applied to flanged beams as they were to rectangular beams. The discussion is analogous to that already given.

MOMENT OF RESISTANCE OF BEAMS DETERMINED GEOMETRICALLY.

The following method of obtaining the moment of resistance of beams is of easy application, and in many cases of unsymmetrical cross section is the simplest that can be used:

I. For illustration, take a beam of rectangular cross section. Let G P (Fig. 63) be the cross section at some point of

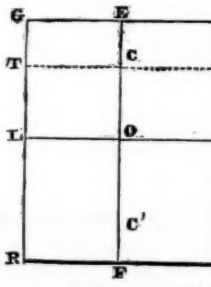


FIG. 63.

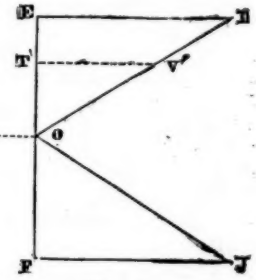


FIG. 64.

this beam. The stresses on the fibres, as we have already seen, increase just in proportion as we go from the neutral axis towards the upper or lower surface of the beam, and may for any vertical slice (as that at E F) be represented by the ordinates of two triangles, as shown in Fig. 64, where E I (=F J) represents the stress on the outside fibre. For the cross section G P (Fig. 63) the stresses will be represented by two wedges, the bases of which are G M and M R, and the elevations of which are the triangles shown in Fig. 64. The volumes of these wedges give the amount of compressive and tensile force exerted at the cross section in question, and the points in G P under the centre of gravity of the wedges give the "centres of resistance," or the points of application of the resultants of these forces.

As a geometrical representation of the stresses on the fibres, these wedges are perfect, for the perpendicular ordinate of the wedge gives in every case the stress which exists in the fibre over which it stands. Thus the line T′ V′ (Fig. 64) represents the stress on each fibre in the row T V (Fig. 63).

But it is often difficult to find the centre of gravity of these wedges in the case of curved and irregular cross sections, and yet this must be done before we can know the lever-arms of the stresses. To render this easier to do we may represent the stresses, not by wedges, but by *prisms*, the centres of gravity of which are over the centres of gravity of their bases.

Thus, if in (Fig. 65) we draw the two

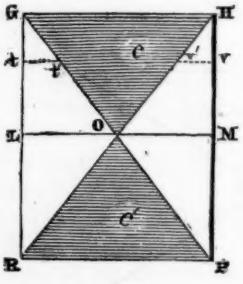


Fig. 65.

shaded triangles, and conceive prisms of a height = E I (the stress on the outside fibre, Fig. 64), to be constructed on them as bases, we shall have a geometrical representation of the stress on the section G P, less perfect in some respects than that given by the wedges but better suited to our purpose.

For, note that,

1. The volume of the prism G O H (Fig. 65) is equal to that of the wedge G M (Fig. 63), and the volume of any part of the prism cut off by a plane parallel to the neutral axis, as that whose base t' O v' is equal in volume to the corresponding part of the wedge T M, or since the height of the prism is constant, the stress on the surface G M as we go out from the neutral axis varies as the area of the triangle which forms the base of the prism.

2. The vertical slice of the prism standing on any line $t'v'$ represents in amount the stress on the line of fibres tv, for this stress is equal to the corresponding one in the wedge, the slice of the prism being as much higher than that of the wedge as tv exceeds $t'v'$. Of course (except in the case of the outside fibres in the row G H) each ordinate in the slice of the prism no longer represents the stress on the fibre over which it stands, as was the case in the wedge.

3. The moment of the tensile forces, for instance, will equal the area of the prism G O H multiplied by its height, (E I = stress on outside fibre = S). The centre of resistance of these forces, or the centre of gravity of the prism is at C (Fig. 65), the centre of gravity of the base G O H. The triangle G O H is sometimes called the "effective area" of

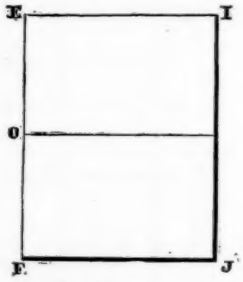


Fig. 66.

the surface G M, because a uniform stress on it of an intensity = the unit

stress at G H gives the same amount of resistance, as that on the whole area G M, acted on as the latter is by a varying stress.

Considering the stresses represented by the two prisms whose bases are G O H and R O P (Fig. 65), as concentrated at the centres of gravity C and C′ (Fig. 67) of these bases, and taking one of these points (as C′, Fig. 67) as the

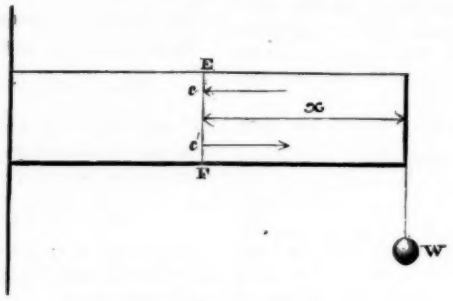


FIG. 67.

centre of moments, we have in the case represented in the figure :

$$(\text{Vol. of prism G O H}) \times \text{C C}' = \text{W}\,x$$

or if b = breadth and d = depth of beam

$$\text{S}\left(\tfrac{1}{4}\,b d\right).\tfrac{2}{3}\,d = \frac{1}{6}\,\text{S}\,b\,d^3 = \text{M} = \text{W}\,x \qquad (65)$$

as before.

Corollary. If the beam be square, $b=d$, and

$$\text{M} = \frac{1}{6}\,\text{S}\,b^3 \qquad (66)$$

II. As a second example, take a square beam so placed that its diagonal will be vertical. Fig. 68 is the cross section. Here we find the base of the prism of

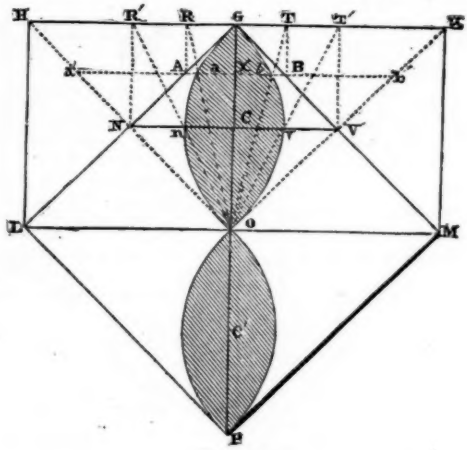


FIG. 68.

stress by points. To find the line in the base of the prism corresponding to the stress in any row of fibres, such as A B, whose distance from the neutral axis is O X, proceed as follows :

We see that if the cross section were the square of which H L M K is the half, then $a'b'$ would be the line required, since this is the breadth at that point of the triangle H O K, which would in that case represent the base of the prism of stress. Project the points A and B

upon H K. Then the actual row (A B) of fibres is as much shorter than the corresponding row in the supposed section H M, as R T is less than H K, and consequently to obtain the proper line in the base of the true prism of stress, $a'b'$ must be shortened in this proportion. Draw lines from R and T to O. These lines intersect the row of fibres at a and b. Then

$$\text{H K} : \text{R T} (=\text{A B}) :: a'b' : ab \quad (67)$$

Hence ab is the line required, and a and b are two points in the outline of the base of the prism of stress. Any number of lines as nv, &c., may be gotten similarly and the curve drawn through the points $a \cdot\cdot n \cdot\cdot b \cdot\cdot v$, &c., will give the form of the base of the prism of stress. This base is shaded in the diagram.

For any ordinate of the curve O n G, as a X, we have

$$\text{O X} : a\text{X} :: \text{O G} : \text{R G} = \text{A X}$$

But $\text{A X} = \text{X G}$ and making $\text{G O} = \frac{d'}{2}$ and putting $\text{O X} = x$ and $a\text{X} = y$, we have

$$x : y :: \frac{d'}{2} : \left(\frac{d'}{2} - x\right)$$

$$\therefore y = \frac{2}{d'} \cdot \left\{ \left(\frac{d'}{2} - x\right) x \right\} \quad (68)$$

This is the equation of a parabola with vertex at n, half way between H K and the neutral axis. Hence the base of each prism is composed of parts of two symmetrical parabolas.

Areas of the bases. Since the area of a parabola is *two-thirds* of the circumscribing rectangle, the area of each base

$$= \tfrac{2}{3} (\text{G O} \times n v)$$

But $nv = \frac{1}{2}\text{R}'\text{T}'$ and $\text{R}'\text{T}' = \frac{1}{2}\text{H K}$

$$\therefore nv = \tfrac{1}{4}\text{H K} = \tfrac{1}{4} d'$$

$$\therefore \text{Area} = \tfrac{2}{3} \cdot \frac{d'}{2} \cdot \frac{d'}{4} = \frac{1}{12} d'^2$$

The centres of gravity of these bases (and consequently of the prisms) are at C and C′, and the distance

$$\text{C C}' = \tfrac{1}{2} d'$$

Hence the moment of resistance of the fibres about C or C′ is

$$\text{M} = \frac{1}{24} \text{S} d'^3 \quad (69)$$

(S = height of prism or stress on external fibres at G and P.)

Corollary. To compare the resistance of the beam in this position with its resistance when lying flat:

Let d = side of the square as L G.

Then $d' = d\sqrt{2}$ and eq. (69) becomes

$$\text{M} = \frac{1}{12} \text{S} d^3 \sqrt{2} = \frac{1}{6\sqrt{2}} \text{S} d^3 \quad (70)$$

Comparing this with eq. (66), we see that the beam offers greater resistance when flat in the proportion of

$$\frac{1}{6} : \frac{1}{6\sqrt{2}}$$

In solving these problems with diagonally placed beams, place the above value of M equal to the moment of the weight as before.

III. Let us apply this method to a T beam. Take for example the cast-iron ⊥ beam, calculated in part on p. 257 Rankine's Civil Engineering, in which the area of the flange = $\frac{3}{5}$ that of the web. Assume the flange to be 6 inches by 1 inch, and the web to be 5 inches by .8 of an inch, and draw a figure of the cross section to scale (Fig. 69).

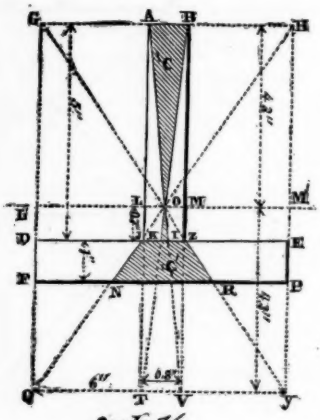


FIG. 69.

1. As the top and bottom of this section is not symmetrical, it is necessary to find the position of the neutral axis, which is no longer at the half-depth. This may be done by calculation, or by a simple mechanical process as follows:

The centre of gravity of the cross section, since this last is symmetrical with regard to the vertical line through the middle of the web, must lie on this line. Cut accurately the figure of the cross section out of card board, or tin, or good paper, and suspend it freely by one end of the flange, suspending also from the same point a plummet. Mark the line of the plummet on the card board, and the centre of gravity being on this line and also on the middle line of the web, will be at their intersection O. By measurement this point was found to be distant from A B four and three-tenths inches (4 . 3″), which is also the value by calculations. The line L M drawn through this point is the neutral axis.

2. To determine the bases of the prisms of stress. On the upper side the base is the triangle O A B, if the altitude be taken equal to the stress on the fibres along A B. For if L′ M′ H G were the upper half section, O G H would be the base of the prism and O A B is less than O G H, in the same proportion that L M A B, the real half section, is less than L′ M′ H G. Hence (if the upper be the compressed side) the total compressive force is equal to the prism erected on A O B, with the height equal to the unit stress at A B.

Below the axis L M.—For convenience we should have the height of the tension prism equal to that of the compression one, and the base must be determined under this condition. Complete the large rectangle G H Q Y, making the distance of Q Y below O=4 . 3 inches. Draw O Q and O Y and the shaded trapezoid J N R Z cut out on the flange by these lines, will evidently be the portion of the base due to the flange. Having prolonged the lines of the web to T and V, draw O T and O V, and then the shaded triangle O K I will be that part of the base due to the portion (L Z) of the web below the neutral axis.

The total tensile and compressive forces being always equal, and the height of the prisms having been assumed, each =S′= stress on fibres at the distance of A B from O, the bases of these prisms must be equal also. This necessary equality between the area of O A B and that of O K I+J N R Z, affords a means of testing the accuracy of our work in finding the position of O.

3. Area of base O A B. This is,

$=\frac{1}{2}$ (L A×A B) $=\frac{1}{2}(4.3\times.8) = 1.72$ sq. inches.

4. To determine the distance C C′ (Fig. 69) between the centres of gravity of the prisms, which distance is the lever-arm to be used when one of these points is taken as the centre of moments. These centres of gravity (C and C′) can be readily determined by means similar to those employed in finding the centre of gravity of the cross-section itself. Thus, cut the shaded areas (Fig. 69) out of card board or paper, and suspending each of them from two points in succession, draw vertical lines through the points of suspension. The intersection of these two lines gives the centre of gravity. In the present case they may be so simply obtained by calculation, that we adopt that method. The centre of gravity of A O B is $=\frac{2}{3}$ the distance from O to A B or O C$=\frac{2}{3}(4.3) = 2.87$ inches. As to the shaded part below O, by using the ordinary formula for the centre of gravity and taking moments around O, we find the distance

$$O C' = \left\{ \frac{O N R \times \frac{2}{3}(1.7) - (O J K + O I Z)\frac{2}{3}(.7)}{O N R - 2 (O J K)} \right\}$$

$$= \frac{2.2764 - .01388}{2.0145 - .2975} = 1.318 \text{ inch.}$$

Hence the distance

$$C C' = O C + O C' = 2.87 + 1.318 = 4.18 \text{ ins.}$$

Hence, since S′ is the height of the prisms, the moment of resistance of the fibres is

$$M = 4.18 \times 1.72 . S' = 7.19 S'$$

If it be desired to have M, not in terms of S′, the stress along A B, but of S″ the stress on the lowermost fibres (at F P), we have since the stresses increase directly with the distance from O,

$$S' : S'' :: L' G : L' F :: 4.3'' : 1.7''$$

$$\therefore S' = 2.53 . S'' \text{ and } M = 18.19 . S''$$

IV. As an illustration of the great saving of labor sometimes effected by this process, take the steel rail now widely used in England, the cross section of which is given to scale in

(Fig. 70). The determination of the moment here by calculation would be long and tedious. The dimensions of the cross section are given on the figure.

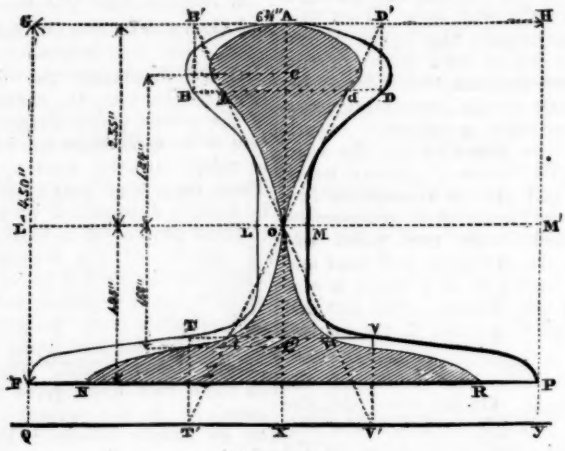


FIG. 70.

1. The centre of gravity O of the cross section is found by making a template as in the last case, and suspending it freely by a corner. The vertical through the point of suspension intersects A X at O, which is 2.55 inches below A. Through this point draw L M, the neutral axis.

2. The bases of the prisms of stress are determined by points as in example II.

Lay off O X=2.55 inches. Draw the rectangle G H Q Y. Assume the height of the prisms to be the stress in the fibre at A. Then proceeding as in example II., the line of the base corresponding to any row of fibres, as B D, is *b d*. Obtain any number of points in the same way as *b* and *d*, and through these points draw a curve bounding the shaded figure A *b* O *d*. Similarly below the neutral axis, *t v* is the line in the base of the stress prism corresponding to T V, and the shaded figure, O N R, is that base where the height is taken equal to the unit stress at A. The equality of the bases in area is the test of accuracy.

3. To determine these areas. The simplest plan in the present case is first to find the area of the cross section itself. This is done as follows: The rail in question weighed 84 lbs. per yard, and the steel, of which it was made weighed, .277 lb. per cubic inch. Hence if A=area of cross section in square inches

(36 . A) .277=84 ∴ A=8.4 sq. inches.

Now cut out of the same card board, or paper, templates of the two shaded parts in the diagram, and also of the cross section itself, and weigh them. The ratio of the weights will equal that of the areas.

The comparison of weights may be readily made by means of a suspended wire, which may serve as a temporary balance, the templates to be compared being stuck on the opposite ends, and one or both moved until the wire is evenly balanced. The weights of the templates being inversely as their distances from the point of suspension of the wire, their areas will be in the same proportion. The areas of the prisms in the case before us were found to be equal each to 2.49 square inches.

4. The centres of gravity of these bases are found in the same way as those of the cross section itself. The point C was thus found to be 1.84 inches above. O and C′ to be 1.66 inches below it. Hence the distance

C C′=3.5 inches.

Therefore, finally, if S′=unit stress at A, the moment of resistance is,

$$M=2.49\times3.5\times S'=8.715.\ S'$$

If we desire M in terms of S″ (stress at F P), we have

$$S' : S'' :: 1.84 : 1.66$$

$$\therefore\ S'=1.11\ S''\quad \therefore\ M=9.67\ S''$$

V. A circular cross section (Fig. 71). Here the neutral axis of course=L M, passing through the centre. Draw the circumscribing rectangle G Y and obtain the points t, b, v, d, &c., in the curve bounding the base, as heretofore. Through the points so found draw the curves.

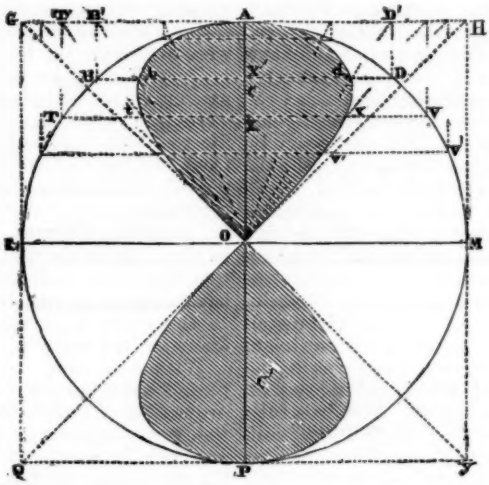


FIG. 71.

Determine the areas of the bases of the stress prisms by comparing them with the half-square G L M H. Thus, if a template is not to the surface of one of these beams it will just equal in weight the template cut to the surface A G L O t b A, or, in other words, the shaded surface A b O d A is just one-third of the rectangle G M, or $\frac{1}{3}.\frac{d^2}{2}=\frac{1}{6}d^2$.

The centres of gravity C and C′ are found as before by means of templates. In this way it was found that

$$O\,C=O\,C'=.587\ (O\,A)=.587.\frac{d}{2}$$

$$\therefore\ C\,C'=.587\,d$$

The height of the prisms being S (= stress at A or F) the moment of resistance is,

$$M=S.\frac{d^2}{6}(.587\,d)=.0978\,d^3\,S$$

(The accurate value by calculation is $M=.0982\,d^3\,S$.)

Note.—The curve of the base of the prisms is a *lemniscate*. To find its equation we have in the triangles O b X′ and O B′ A,

$$b\,X' : A\,B'\ (=B\,X') :: O\,X' : O\,A$$

And taking the vertical axis as that of X, and the horizontal one, as that of Y, the origin being at O, and calling the coordinates of the circle x' and y', and those of the *lemniscate* x and y we have:

$$y : y' :: x : R,\ \text{or}\ y : \sqrt{R^2-x^2} :: x : R$$

$$\therefore\ R^2y^2=x^2\,(R^2-x^2).$$

PRESERVATION OF METALLIC SODIUM.—According to Bottger, if sodium be placed in alchohol until its surface becomes brilliant, and then in naphthalic ether chemically pure, and finally in a concentrated solution of naphthaline in naphthalic ether, the metal may be preserved unalterable with its lustre unimpaired, for a long time.

THE CONVERTED RODMAN GUN.

From "Engineering."

We have recently referred to experiments soon to be made in the United States with some new forms of ordnance, among them being a cast-iron Rodman gun converted upon the Palliser system. Some interesting trials with this gun have been already carried out, and their results are so encouraging that the Ordnance Board have recommended that more of the cast-iron guns now forming the United States heavy armament should be converted in a similar manner.

The gun with which the experiments just referred to were carried out was originally a 10-in. smooth bore cast-iron piece. The bore was enlarged to a diameter of 13.5 in. and a wrought-iron tube 2.75 in. thick was introduced. The cast-iron shell had been made in 1866, the ultimate tensile strength of the metal was 32,369 lb. per inch, and the initial tension on the gun was 12,000 lb. The iron tube was manufactured by Sir William Armstrong & Co., and the clearance between the outside of the tube and the shell was about $\frac{1}{100}$th of an inch.

The gun had originally been made without preponderance, and by its conversion it became heavy at the muzzle. This would be the case with all the similar guns so converted, and it is proposed to overcome the difficulty by reducing the diameter of the trunnion from 10 in. to 8 in., removing the metal eccentrically, and then shrinking eccentric rings over the trunnions until their diameter is restored, and the centre of gravity is brought into the required position. A collar at the muzzle keeps the tube in the gun, and a screwed plug prevents it from turning. The rifling consists of fifteen grooves of equal width with the lands, the rate of twist being 1 in 40. The powder employed was that known as the double hexagonal grain, to which we referred on a recent occasion. The specific gravity is 1.7511, and the weight equals 80 grains to the pound. The dimensions of the grains are as follows: Width between faces of cones .7 in., width over all .75 in., width of faces at each side .32 in., thickness of parallel portion between bases of cones .15 in.

Two classes of projectiles were employed for the trial, known as the Butler and Arrick projectiles—to the former we shall refer again shortly with considerable detail—but only sixteen rounds were fired with the latter class.

The following are some of the leading particulars of the trial: Five rounds were fired with charges rising from 20 lbs. to 25 lbs., and weights of projectiles from 160 lbs. to 175 lbs. No change in the tube was detected after these firings.

Seven rounds were then fired with 35-lb. battering charges, and 173-lb. projectiles. After these rounds it was found that the tube had set at some points hard against the cast-iron shell, and that at the point of maximum pressure, the diameter had increased to 8.007 in.

With the same charge 38 further rounds were fired, of which 30 were with projectiles of 186 lb. weight; after these the greatest enlargement of the tube was .002 in. at the charge, and .003 at the projectile. Sixteen rounds were then fired with a different class of projectile, weighing 165 lb.; these gave very bad results, and were discontinued. They were succeeded by 50 rounds with 35-lb. powder charges, and 174-lb. projectiles, with a further enlargement of .001 at a position from 36 in. to 40 in. from the bottom of the bore, the tube at this point not having been previously extended. Sixty-three rounds were then fired, of which 50 were with projectiles weighing 187-lb. No further enlargement was detected, but beyond the trunnions the tube set out .002 in. One hundred additional rounds with 171-lb. projectiles resulted in a further enlargement of .002 in. at a point 24 in. from the bottom of the bore. These were succeeded by another 100 rounds, with similar charges and projectiles, and the increased enlargement was found to be .003 in. One hundred and forty-seven further rounds completed the total of 513, to which the gun has been already subjected, the final series causing an enlargement of .004 in., and the total increase in diameter being .018 in., counting from the twelfth round. Of the

above, however, sixteen rounds were fired with unsuitable projectiles, and the effect they produced may be fairly deducted, so that on 484 rounds fired with battering charges the enlargement was .011 in.

In conducting these experiments the velocities were measured with the Le Boulengé chronograph, and the Rodman pressure gauge was also employed. The maximum muzzle velocity was 1,459 ft., the mean maximum pressure with battering charges was 31,282 lb., and the maximum energy of projectile per inch of shot's circumference was 220,346 foot-pounds.

The results thus obtained are, it will be seen, highly satisfactory, and open up an effective mode for the improvement and strengthening of the United States ordnance, of which the cast-iron is the best in the world. The present armament of the United States for coast defence includes 1,294 ten-inch Rodman guns, and in their report the Ordnance Committee point out that these guns are at present useless for purposes of defence against armor-plated vessels, so that the casemates and batteries constructed at an enormous outlay, are comparatively useless, and must remain so, until the present armament shall have been replaced by new guns, or the present ones are converted into efficient rifles.

They consider that the trials already carried out with the 10-in. converted Rodman, are sufficiently encouraging to justify the expectation that by the same process the existing cast-iron guns can be converted into formidable weapons, but before recommending any extensive change, they propose to carry out further trials with another 10-in. and a 12-in. calibre Rodman, similarly converted.

THE FUSION OF STYLES.

From "The Architect."

From being the rallying points of two opposing camps in the architectural profession, the forms of architectural design long known as Classic and Gothic seem to be gradually entering into a strict bond of amity, and on the way to prove to the unprejudiced spectator that they are "not so very different after all." The time has at least gone by when the critic of decided leanings towards one or other school could arrange the sheep of Classicism on one hand, and the Gothic goats on the other. We are losing these sharp distinctions. Collections of architectural drawings, as in competitions, are no longer to be divided into designs with columns and pediments, and those with buttresses and pinnacles. If in such a gathering there is found a set of drawings of which the details are purely Roman, or one that can be safely referred to a particular quarter of the thirteenth and fourteenth century for all its precedents, it is commonly noted by the enlightened observer as "tame," and rewarded with faint praise accordingly; while his warmer commendations are reserved for designs which, spurning these trammels of a past day, present a union, or *réunion*, of features from both the main sources of the modern architect's inspiration; sometimes fortunately combined, sometimes reminding one of Portia's suitor from England, who she thought had bought "his doublet in Spain, his round hose in France, and his behavior everywhere." It would be natural that such a fusion of recognized styles should be practiced for some time by architects before its existence began to be observed outside the profession; but the fact is penetrating the non-professional stratum now; and as we read in a weekly literary contemporary the other day that the Queen Anne designs of the London Board Schools which are being erected are "in all their main qualities essentially Gothic," it is time to look about us.

And, on the whole, we think an impartial survey of the average buildings going on just now will show that Gothic and Classic have not met on quite equal terms, and that the latter is in fact having rather the worst of it, and is more or less succumbing to the Gothic. It is

true we have still a large proportion of buildings in which columns and architraves play an important part, and windows are adorned with little pediments and neat consoles; but the change is shown in these by the increased vivacity, variety, and relief of the carved decoration. No architect, with any concern for his artistic reputation, is content to adorn his building with the old stereotyped festoon and garland kind of thing, with no novelty of idea and no light and shade in execution. Gothic detail has overflowed into our Classic buildings, and carving is found on these which would be nearly as well in place on a building of manifestly Gothic type. But the converse is hardly the case. We do not often find details in any marked degree Classic in character introduced into Gothic buildings. The fluted column, and the facias of the architrave, do not find their way there, and would be pretty well killed if they did. The nearest approach to something of Classic feeling in our modern Gothic buildings is perhaps the use of the heavy square pillar, with carved capital, which may be regarded as a kind of modification of the pilaster; though, in fact, it is found in early Gothic building in the North of Europe especially; and perhaps Classic influence may be recognized also in the increased dimensions and more pronounced character of the horizontal cornice in modern Gothic buildings. It might not be very easy to say whether the leaning towards early and even Romanesque Gothic at present is a cause, or a consequence, of the feeling in favor of a kind of fusion of Classic and Gothic; but we are inclined to regard it as a consequence, and as arising almost unconsciously from a desire to secure the dignity of expression belonging to Classic architecture, and perhaps the fitness of its horizontal composition for practical purposes, without losing the variety and play of light and shadow so characteristic of Gothic work. Thus, in an indirect way, the Classic has influenced the main type of modern Gothic; but in regard to the direct visible effect of the one style on the other, there can be little doubt, as before observed, that Gothic is carrying the day, and is overrunning what would otherwise be Classic design with its own specialties of detail.

This fusion of Gothic and Classic is what has been preached some time back by certain critics, who may now see their ideas carried out to a greater extent perhaps than they ever expected. And perhaps any movement which gives us something in place of mere copyism deserves to be called a gain. But as we look at the buildings of this mixed type of architecture springing up around us we cannot say that it is clear gain. A much greater variety of manner, a (generally) much more ornate treatment we do see than under the old "pure style" method, and occasionally a really picturesque combination of detail resulting in a satisfactory and homogeneous effect. But in the main this homogeneous quality, this unity of treatment, is just what we miss in our new buildings; there seems a want of totality, of relation of parts, in them, accompanied too often also by a lamentable want of that refinement of detail which, after all, intelligent reproduction of a complete and uniform style at least secured to us. Our new buildings seem full of details that are quarreling with each other; full of contradictory emphasis. Carved ornament is crowded on more abundantly than thoughtfully; granite shafts with large spreading capitals are put wherever they can be got in; archivolts and window-heads are bemoulded and recessed so as to be quite over-heavy, and keystones are getting to such a size as to be sometimes double the length of the radius of the arch in which they are inserted. All this produces a certain novelty of effect, and the appearance of a great deal of elaboration of design; but it is, in one word, vulgar; and that is just the character of a great proportion of this nondescript architecturewhich is being produced at present. Details properly Classic would unquestionably look hard and thin if transferred to a Gothic building; but the converse is equally true, that details which would be very well in place in a design entirely and unaffectedly Gothic look large, coarse, and over-pronounced, when transferred to a design the main type of which is Classic. The two schools of detail will not really harmonize with one another, except to a very limited extent. But what is even more marked in the class of design of which we are speak-

ing is the loss of that unity and homogeneous structure which belongs to really good purely Classic or Gothic buildings. In these latter, at least, everything seems to be in its place, and to have a general coherence of treatment and style; a quality the value of which can scercely be overrated. And it is the evident loss of this in recent architecture which tempts one to doubt whether the escape from architectural reproduction, upon which we are sometimes felicitated, is by no means an unmixed gain. Such a building as Whitehall may be considered very artificial and cold in style; no doubt it is, but it has proportion and consistency of design, and is in perfect keeping with itself. And similar praise may be bestowed on a much greater building in every way, the Houses of Parliament. That the style selected for it is not in itself one of the best phases of Gothic is unquestionable; that it might have been treated with more power and effect one may be at liberty to think. But at all events there is a "oneness" about it; an entire keeping throughout, which we look for in vain among the buildings of the eclectic school. It is possible that by means of this experimental medley through which we are passing we may arrive at something both novel and coherent in the end; but it must be confessed there is not much indication of that at present. Novelty, no doubt, there is, plenty of it; but the old dignity of architecture seems to suffer sadly in the interim. And, with every wish that modern architecture should entirely emerge from mere copying, we may suggest that it is paying too dear for this to throw aside and ignore that coherency of style and design which has characterized all that we admire most in the architecture of the past; and that possibly a truer (though perhaps slower and more laborious) road to originality would be found in mastering the feeling and constructive principles of a complete style, and adapting that, freely and unreservedly, to the practical wants of the day, than in picking details out of different styles, in the vain hope of piecing them together into a new and "original" one; original only in the sense of having no origin.

One thing is unquestionable: that if this fusion of styles goes on at the rate which it seems to threaten, the business of architectural critics will become seriously and painfully complicated. It used to be sufficient to describe a building as Greek, Roman, or Gothic; everybody knew what was meant. But now the varieties of combination are so diverse and unexpected, that language fails in the effort to describe them in any concise form. The newspaper writers have been hopelessly bothered for some time back, and do not know what to call a new building now; and the professed architectural critic must succumb before long, and must either invent a new and very extensive vocabulary, or call upon the architect in each case to say what he wishes his design to be considered.

SPONGY IRON.

From "Iron."

At a recent meeting of the Newcastle-upon-Tyne Chemical Society, held in the theatre of the College of Physical Science, Mr. J. Pattinson, the President, in the chair, Mr. Gibb read a paper on "Spongy Iron."

Spongy iron, or iron sponge—the slightly cohering mass resulting when iron ores are reduced below the welding heat of iron, said the author—is produced in nearly all iron smelting processes, although it is a form of iron but little known in practice. Many proposals have been made to separate iron smelting into the two distinct operations of reduction to sponge, and the subsequent welding or melting of this product to produce malleable iron, steel, or cast iron. The earliest attempts on a large scale were those of Clay, under patents obtained in 1837 and 1840, and he has been followed by a series of inventors, of whom Chenot conducted the most elaborate recorded experiments. Efforts in this direction still continue; two processes, having for object respectively the manufacture of

puddle bar and steel, being now carried on on a somewhat large experimental scale ; and lately a series of extensive experiments were made by Siemens with specially planned furnaces that certainly reduced the ore to sponge, and melted the latter successfully so far as economy of working was concerned. But iron as sponge is in a most favorable condition for absorbing sulphur from the reducing agent and from furnace gases—a drawback that compelled Siemens to abandon this method of working.

Although the separate production of spongy iron, for the manufacture of iron does not give workable promise, the fine state of division of the metal in the iron sponge renders it very suitable for the precipitation of copper from solutions produced in the extraction of copper from its ores by the wet method.

For the use of spongy iron, in the reduction of sulphides, &c., in the dry way Bronæ and Deherrypon obtained a patent in 1859, and later in the same year Gossage patented the use of spongy iron, reduced from burnt pyrites in ovens or muffle furnaces, for the precipitation of copper from solution. In 1862 Bischof patented the manufacture and application of spongy iron for copper precipitation, his process and raw material being essentially the same as those described in Gossage's patent three years earlier. In 1863 Bischof patented an arrangement reverberatory furnace and accessory apparatus, for the production of spongy iron, for use in precipitation and other purposes. Henderson in 1863 and 1867, patented a variety of furnaces, and in 1869 Snelus patented a furnace similar to Gerstenhofer's pyrites kiln, for the production of spongy iron, but no one of their devices has been adopted. Proposals patented later than Bischof's have had, for main object, the production of sponge for the manufacture of iron and steel. Most of these have been some form of retort or muffle furnace in which the mixture is heated by transmission through brickwork, the retort being horizontal or vertical. This method is slow in action, and the wear and tear of the brickwork has proved too great in practice. Snelus' furnace, in which the finely-ground material falls from one series of bars to another in a reducing atmosphere, whilst maintained at a red heat, appears well adapted for the production of sponge, but its introduction being proposed for the manufacture of steel, the liability of iron in this state to absorb sulphur from furnace gases would probably prevent its adoption.

Siemen's cylindrical revolving furnace, although well adapted for quick and economical reduction, was abandoned for this reason. The vertical retort furnace has been again proposed by Blair, who states that he has overcome the former difficulties of working this class of furnace. In a vertical retort externally heated, unless the width be impracticably small, an excessive time is required for heating the mass through. Blair employs a shaft about 4 feet in diameter, and by the device of a cylinder, suspended in the throat and leaving an annular space only three or four inches wide, and heated internally by gas at the same time that the external shaft is kept heated, the ore enters the body of the furnace at a red heat, which is then readily maintainable in the mass. The reduced iron, passing down into a cooling shaft, is withdrawn from time to time whilst fresh ore and charcoal are charged into the annular mouth.

Only one form of furnace is now employed in making iron for precipitation. This is essentially a reverberatory furnace 30 feet long, with provision for conveying the flame under the hearth after it has passed over the charge. The hearth of the furnace is 23 feet long and 8 feet wide, and is divided into three working beds by bridges. Each bed has two working doors on one side. The doors slide in grooves and close air-tight. The fire is 4 feet by 3 feet, with bars 4 feet 8 inches below the bridge, thus allowing for a considerable depth of burning fuel. The fire door slides in grooves like the working doors. The hearth is formed of tiles sustained on brickwork partitions forming flues through which the flame returns after passing over the hearth. From these flues the flame drops, by a vertical flue alongside the fire-bridge, to an underground flue, communicating with a chimney. The entrance to the latter flue is provided with a fire-tile damper, which is closed whenever the working or fire doors of the furnace have to be opened. A cast-iron pan, 20 feet by 10 feet, is carried by short column

and girders over the furnace roof. In this pan the ore is dried and mixed with coal, and from it is charged into the hearth, through cast-iron pipes, built into the furnace arch. The furnace is elevated on brick pillars, to allow of iron cases running under it, to receive the reduced iron, and it is worked from a platform of cast-iron plates. A vertical pipe, 6 inches diameter passes through the hearth of the furnace inside of each working door, and through these pipes the reduced iron is discharged into iron cases placed beneath. These cases are horizontally rectangular and taper upwards on all sides. The cover is fixed and in its centre is a hole 6 inches diameter, with a flange upwards, which serves to connect the discharging pipe. The bottom of the case is closed by a folding door, hinged on one side, and secured by bolts and cutters on the other. The case is fitted with four wheels, clear of the door, and is covered with a cast-iron plate, fitting loosely into the opening on the upper side. It stands four feet eight inches high, and has a capacity of twelve cubic feet.

The furnace hearth being at a bright red heat, each of the three working beds is charged, with 20 cwt. dry purple ore and 6 cwt. ground coal, from the cast-iron pan over the roof. The fire and working doors are closed, and the only air entering is that through the fire, in working which care is taken to prevent the mass of burning fuel getting hollow. The charge in the first bed from the fire-bridge is reduced in from nine to twelve hours; in the second, in eighteen hours; and in the third, in about twenty-four hours. Each charge is stirred over two or three times during the period of reduction. Before opening any door the flue damper is closed, to prevent a current of air entering over the charge. On the complete reduction of the charge, on any working bed, two cases are run under the bottom pipe, to which their mouths are luted by clay, and the charge is quickly drawn into them, by rakes worked through the doors. The cases are then removed and closed with cast-iron plates. In about forty-eight hours the iron is cooled sufficiently to be discharged, and this is simply done by raising the case by a crane, and knocking out the cutters fastening the hinged door on the bottom, when from the tapering form of the case the mass of reduced iron falls out readily. The sponge is ground to powder under a pair of heavy edge stones, 6 feet in diameter, and is passed through a sieve of fifty holes per lineal inch.

For the manufacture of spongy iron for precipitation, two materials have been proposed, viz., burnt pyrites and "purple ore." The following are analyses of these materials:

	Burnt Ore.		Purple Ore.
Ferric Oxide	78.15	..	95.10
Iron	3.76	..	—
Copper	1.55	..	.18
Sulphur	3.62	..	.07
Cupric Oxide	2.70	..	—
Zinz Ozide	.47	..	—
Lead Oxide	.84	..	.96
Calcium Oxide	.28	..	.20
Sodium Oxide	—	..	.13
Sulphuric Acid	5.80	..	.78
Arsenic Acid	.25	.	—
Silicious residue	1.85	..	2.13
Total	99.27	..	99.55

Bischof and Gossage both proposed the use of burnt ore on the ground of the obvious economical advantage that the copper it contains is obtained, with the precipitated copper, without the expense of extraction. But burnt ore contains a notable portion of arsenic—.16 per cent. in above analysis—and this metal remaining in the sponge, is left mixed with the precipitated copper, and seriously deteriorates the quality of the refined copper ultimately made from it. Bischof states that, "should the ore have contained traces of metals, such as arsenic or lead they will be volatalized during the process of reduction." Whilst lead is reduced and in a great measure volatalized in the spongy iron furnace, the arsenic in such ores being present mainly as arseniates of copper and iron, which are likely to be reduced to fixed arsenes, is not volatilized, spongy iron made from burnt ores containing a proportion of arsenic closely agreeing with that in the ore. "Purple ore," which retains only the most minute trace of arsenic, is the only material now employed, and the following analysis gives the composition of spongy iron, made from purple ore by means of the furnace and method described above:—Ferric oxide, 8.15 per cent.; ferrous oxide, 2.40; metallic iron, 70.40; copper, .24; lead .27;

carbon, 7.60; sulphur, 1.07; alumina, .19; zinc, .30 ; silicious residue, 9.00 ; total, 99.62 per cent.

In using spongy iron in precipitating copper, the liquids are agitated by an air blast whilst the iron is gradually added. By this means a very perfect mixture is obtained, and a copper precipitate can be readily produced, containing not more than 1 per cent. of metallic iron. As compared with precipitation by scrap iron, the economy of space required and facility of manipulation are very great. On the side of spongy iron precipitation are cheapness of material and economy of application ; whilst against it is the presence with the precipitated copper of the unreduced iron oxides and excess of carbon from the reduction. In employing spongy iron, the copper extractor has the production of the precipitant in his own hands, and avoids the troublesome handling of a material so cumbrous as scrap iron.

As regards the chemistry of spongy iron precipitation, it is, of course, identical with that of scrap iron precipitation, and although it is stated by Bischof that, "some substances, such as arsenic especially, are only precipitated after the iron has been in contact with the solutions containing copper and these substances for several hours. The precipitation of the copper by my process being finished in a much shorter time, and the solutions then being separated from the iron powder, the above substances cannot be precipitated or mixed with the precipitated copper," the writer has been unable, with iron in any form or with copper solutions in any state, to completely precipitate copper and leave any, even the smallest, proportion of arsenic in solution.

The president says that a paper coming from such an authority as Mr. Gibb, was very valuable. From his experience of the temperature at which spongy iron was formed from oxide of iron, he wished to know whether it could be formed below a red heat, or was a continued red heat necessary for the formation of metallic iron ?

Mr. Gibb replied that he could hardly say. They took care to keep their furnace at least at a red heat, yet it was often worked at a very dull red heat at the flue end of the furnace, and the best iron was then made by a long way—the best iron for precipitation purposes; but it was simply a question of time; and it would be found that when the furnace went down to such a heat as that, instead of coming out in about twenty-four hours they were bringin g it out in about sixty or sixty-two hours, which was a time quite inadmissible in pratice. But at a very dull red heat the iron was reduced, and reduced thoroughly, to a metallic state, but time must be given.

Mr. Scholefield wished to know whether it was stirred much during that period.

Mr. Gibb : Rarely ; now and again, but not very often. That was one of the things experience had shown should be done as little as possible, because with all the precaution of dampers and close fires and everything of that kind, it could not be stirred without air getting in, and they found the less it was stirred the better. It had to be stirred up or it would cake, particularly over the bed.

Mr. Lomas wished to know the thickness of the layers in those beds; if they perfected the process in each of these three divisions without removal from one end to the other; and if each was perfect by itself ?

Mr. Gibb, in reply, said that the thickness was six inches; that there was no removal, as there was a partition to prevent that; that each was perfect in itself, and that was why he said the bed next the bridge was completed in from nine to twelve hours. The flue bed took about twenty-four hours, and the others something between the two.

The President said, then the higher the heat the more rapid the formation of spongy iron.

Mr. Gibb replied, yes ; that was just the reason why, without having much experience in iron smelting, he doubted whether spongy iron would ever be made unless in such a great structure as a blast furnace, where no doubt spongy iron was made, and in fact no doubt all iron passed through that state, and where they had the quicker action of a reducing gaseous current. That was why he doubted altogether that spongy iron as a manufacture by itself would ever take its stand as a step towards another process, either smelting or puddling, in the manufacture of iron.

ON THE MEANS OF AVERTING BRIDGE ACCIDENTS.

Transactions of the American Society of Civil Engineers.

REPORT I.

To the American Society of Civil Engineers:

The Committee appointed, under the resolution of May 21st, 1873, * to inquire into the "most practicable means of averting bridge accidents, begs leave to report as follows:

After a careful examination into the causes of the most disastrous accidents which have occurred during the past few years, it finds that they can readily be divided into three different classes. First, where bridges are erected by incompetent or corrupt builders, and accepted by incompetent or corrupt railway or municipal officials. Second, where bridges of good design and sufficient material fail from absolute neglect on the part of their owners, or from injury to the material during transportion or erection. Third, where bridges, good or bad, are knocked down or destroyed by derailed trains moving at a high rate of speed, or where the growth of a neighborhood has brought a class of traffic on a bridge, which it was not originally designed to bear, either by the builders or the owners. How to treat each of these classes of causes will now be considered in the order above stated.

Accidents occurring from the first class would certainly not have taken place had the wrecked structures been correctly designed and had they possessed the proper sectional areas in their different parts—but failure from faulty design, is not nearly so frequent as failure from insufficient material. One great difficulty in the way of protecting the public from the results of imperfect design or scanty material lies in the absence of a fixed legal standard of loads and stresses for all classes of these structures, and another is the negligence of those controlling public works or those engaging in their construction, in securing skillful professional aid.

It would seem, therefore, to be our duty as a Society to establish in a few general terms—such as can be readily embodied in a law—a standard of maximum stresses and a table of least loads for which bridges should be designed, and to add thereto a practicable suggestion as to the necessary legislation required to give the public that protection which an adherence to this standard would afford. First, as to the standards for the least live loads to be used in proportioning bridges; a law which would provide that all railroad bridges should be built to carry not less than the following loads, would be well within the mark of safety.

For highway and street bridges the standard loads should not be less than as in first table on next page; for city and suburban bridges and those over large rivers where great concentration of weight is possible, as in column *A*; for highway bridges in manufacturing districts, or on level, well ballasted roads as in column *B*, and for country road bridges, where the roads are unballasted and the loads hauled are consequently light, as in column *C*.

Spans.	Pounds per square foot.		
	A.	B.	C.
60 feet and under...	100	100	70
60 to 100 feet.....	90	75	60
100 to 200 feet.....	75	60	50
200 to 400 feet.....	60	50	40

With the highway bridge the floor-beam strength is especially important, because of the great concentration of weight which may be carried on a single pair of wheels, therefore the floor

* At the Fifth Annual Convention, held at Louisville, Ky., May 21st and 22d, 1873, it was:—

"Resolved: in view of the late calamitous disaster of the falling of the bridge at Dixon, Ill., and other casualties of a similar character that have occurred and are constantly occurring, that a committee * * * be appointed to report at the next Annual Convention on the most practicable means of averting such accidents."

The committee appointed consists of Messrs. James B. Eads and C. Shaler Smith of St. Louis, Mo., I.M. St. John of Quinnimont, Va., Thomas C. Clarke of Philadelphia, Pa., James Owen of Newark, N. J., Alfred P. Boller, Octave Chanute and Charles Macdonald of New York, Julius W. Adams of Brooklyn, N. Y., and Theodore G. Ellis of Hartford, Conn. Mr. Alfred L. Rives of Mobile, Ala., was appointed on the Committee but resigned.

system of each class of bridge should be —per floor-beam for each wagon-way— for city bridges, 6 tons; turnpike bridges 5 tons, and county bridges, 4 tons.

Span or Panel.	Pounds per Lineal Foot of Track.	Span or Panel.	Pounds per Lineal Foot of Track.
Under 12 feet.	6,000	Under 75 feet.	3,000
Under 15 feet.	5,500	Under 100 feet.	2,750
Under 20 feet.	5,000	Under 150 feet.	2,500
Under 25 feet.	4,500	150 to 175 feet.	2,500
Under 30 feet.	4,000	175 to 200 feet.	2,400
Under 50 feet.	3,250	200 to 300 feet.	2,250

The panel weights for railroad bridges are obtained by using the standard weight per foot for short spans. In computing all web members, one panel of panel weight is to be considered as preceding the standard span load. The proposed law should also provide that with the foregoing loads, the stresses or materials shall not exceed the following:

For wrought-iron in tension, long bars or rods.....10,000 pounds per square inch.
For wrought-iron in tension, short links (for floor beams)..... 8,000 pounds per square inch.
For wrought-iron against shearing force..... 7,500 pounds per square inch.

And for wrought-iron in compression, as in this table:

Diameters.	Pounds per Square Inch.		Diameters.	Pounds per Square Inch.	
	Square Ends.	Round Ends.		Square Ends.	Round Ends.
10	10,000	7,000	30 to 35	6,000	4,000
10 to 15	9,000	6,500	35 to 40	5,000	3,500
15 to 20	8,000	6,000	40 to 50	3,800	2,500
20 to 25	7,500	5,500	50 to 60	3,000	2,000
25 to 30	6,800	5,000	——	——	——

Where one end is square and the other end is rounded, a mean is to be taken between the two.

Cast-iron to be used in compression only, in lengths not exceeding 22 diameters, and at the same stresses as those prescribed for wrought iron.

The shapes under compression in the above are assumed to be hollow struts either square or cylindrical in section; other shapes than these to have the stresses varied as actual experiment may dictate.*

For wood the greatest allowable strains shall be as follows:

For oak in flexture...1 200 lbs. per square in.,
" pine " ".....1 000 " " " "

and in compression as in this table:

Diameters.	Pounds per Square Inch.	
	Oak.	Pine.
10	1,000	900
10 to 20	800	700
20 to 30	600	500
30 to 40	400	300

The above standard should be changed or elaborated more fully, from time to time, as future experience and experiments on material suggest.

In order to secure to the public the full measure of benefit from the adoption of this standard, the law in question

* Further experiments can alone determine the values to be used for other than square or cylindrical cross sections.—A. P. B.

should provide for the appointment by the governor of each state, of an expert whose duties would consist in having cognizance of the construction and maintenance of every bridge intended for public travel in the state or states for which he was appointed. The law should also make it imperative that the expert so appointed shall pass an examination as to his mathematical and mechanical competency, which it is suggested, should be by a standing committee of this Society, regularly constituted for the purpose, and that the appointment of any such expert who fails to receive the endorsement of this committee shall be null and void. Under the proposed law it should be the duty of all railroad, city, county or state officials having charge of the letting or construction of bridges to call upon this expert—first, to examine the strain-sheet of the proposed structure before work has been commenced, to certify to its correctness if correct, or to make such alterations as may be necessary if it is faulty in design or scant in material according to the legal standard ; next, to be present on the completion of the bridge, and then and there to make a critical examination of the work in all its details, comparing and verifying the sections on the strain sheet with those of the actual structure, and if these last are insufficient, to forbid the use of the work until the law is fully complied with ; and lastly, if the bridge is up to the standard in all its parts, to obtain from him a certificate to that effect, copies of which certificate shall always be given to the builder, and filed on record in the proper department of the state government. This officer shall also see that a tablet or plate is placed on a conspicuous part of the bridge, bearing the names of the builders, his own name, and that of the officer of the railway or corporation who accepts the work, together with the strength of the bridge as designed, and the year of its erection.

Accidents arising from the first class of causes would be nearly, if not quite prevented by the general enforcement of the foregoing provisions. Against accidents occurring from causes of the second class, the law should further provide that all railway or other corporate bodies, when having a bridge built, to be used for public travel, shall be compelled during the erection of the work, to keep on the spot a competent inspector, who shall have the power to reject any piece of material which may have been injured in transportation or while being placed in position. Also that all railroad and city bridges shall be inspected once every month by a competent person in the employ of the corporation owning the bridge, for the purpose of seeing that all iron parts are in order, all nuts screwed home, that there are no loose rivets, that the iron rails are in line and without wide joints, and that all wooden parts of the structure are sound and in proper condition.

It should also be the duty of the state officer before mentioned, upon any bridge being reported as in a neglected condition—whether the report be an official one or made by one not connected with the corporation—to proceed to the spot and examine for himself, and if he finds the bridge in a neglected or dangerous condition, he should cause the owners to put it in safe order without delay.

In relation to the third class of causes —destruction by derailed trains, high winds, or by concentration of living weight owing to the growth of cities or neighborhoods—prevention is less easy, but much can be done by carefully designing the structure. In most of our railroad bridges the floor system is the weak point. The cross-ties are short, the stringers are proportioned for a train *on*, not *off* the rails; and the guard-timbers are too low, and are insufficiently bolted. A derailed engine on such a floor as this, plunges off the end of the cross-ties into the open space between the stringers and the chords, and generally wrecks the bridge. To obviate this, the law should provide that, first,—all cross-ties shall extend from truss to truss, they shall be placed so close to each other that if supported at the proper intervals it will be impossible for a derailed engine to cut through them, and the stringers shall be so spaced as to give them this support. Next, the guard-timbers shall be scantlins not less than 9×10 inches, and they shall be strongly bolted or spiked to at least each alternate cross-tie. And lastly, the clear width between the trusses on through bridges shall be so great that the wheels

of a derailed train will be arrested by the guard-rail before the side of the widest car can strike the truss. Where switches are placed at the end of a bridge, the Wharton or some other form of safety switch should be used.

Against the majority of accidents from high winds, a provision in the law requiring that all lateral bracing shall be sufficient to resist a pressure of 30 pounds per square foot of truss and train, will be sufficient. Lateral bracing can be proportioned at 15,000 pounds per sqnare inch against this particular strain, as it is of very rare occurrence.

The last case in the third class of causes of accidents is where a bridge built originally for a neighborhood or country road becomes too weak for the requirements of a growing community or possiby of a newly established manufactory; also where a railroad bridge, intended only for that class of traffic, has a highway floor subsequently added to it. Against the first contingency, the vigilance of the state official and a chance that some of the users of the bridge may occasionally notice the tablet setting forth its strength, would seem to be about the only safeguard; but in the second case the law should provide that—except by permission of the state officer in charge of bridges—no corporation or other bridge owner shall add to the dead weight on a bridge without at the same time making the proper addition to its strength.

The foregoing provisions, if embodied in a law, will afford the public about all the protection which is readily obtainable in the case.

No mention is here made of the quality of the material, as the proposed officials engaged in carrying out the law will be men who have been passed on by the Society, and the very fact of their surveillance will be apt to produce care in this regard. In addition to this, the standard stresses have been placed so low that the use, whether accidental or fraudulent, of low grades of iron will hardly endanger the work. A provision in the law that all bridge details shall possess the proper proportional strength to that of the main members of the bridge, and a series of instructions from the examining committee of the Society to those who pass their examinations for appointments under this law in reference to these proper proportions, will protect the purchasers of bridges from insecure details of construction.

In addition to his duties, as above defined, the state officer in charge of bridges should also visit the scene of any accident in his district as soon as possible after the occurrence, and remain during the removal of the wreck, or until he is able to ascertain the true cause of the failure. The facts in the case should then be reported by him to the examining committee of the Society.

In conclusion, it is here advised that a committee be appointed to draft such a law as is outlined in this report; that a resolution be passed by the Society recommending the adoption of this law by the different state legislatures, and that printed copies of this report, the proposed laws and the accompanying resolutions, be sent to the members of the Society with a request that they move actively, each in his own state, towards procuring the passage of the specified law by the various state legislatures during the coming winter.

JAS. B. EADS, Chairman.

Oct. 30th, 1874. C. SHALER SMITH. *

* In advocating the views presented in the foregoing report, the undersigned is actuated by the following reasons.

First—the resolution under which the Committee is acting requires from it "the most practicable means of averting—i. e., preventing bridge accidents," rather than the mode of sitting in judgment on them after they occur.

Second—as the national legislature has for some time been passing laws for the protection of life on navigable waters of the United States, prescribing qualifications and standards for engineers and pilots, the proportions of safety valves, &c., for boilers, and appointing examiners and inspectors under these laws, so, sooner or later will the question of the proper construction of railways be taken up and legislated upon.

Third—many mistakes have been made in these laws, owing to ignorance on the part of those passing them, and the undue influence of interested inventors and manufacturers, and each succeeding Congress has had amendments to make in order to repair some injustice or supply some omission.

Lastly—as laws regulating the construction of railroads and bridges will certainly be enacted, and official positions will assuredly be created by them, it is far better that this Society should take time by the forelock, dictate a law which will be just and equitable, and hold control of the appointment under it, than that it should stand in the background, until an aroused public opinion compels legislation which may be injurious to the profession, especially if enforced by political appointees who may be utterly unfit to fill such positions. All laws are written by some one, and the greater the knowledge of the subject matter on the part of that person is, the more probable the production of a good and wise statute.

Hence the undersigned believes that the fixing of the standards as proposed, the preparation of such a law as suggested, and the professional surveillance of the appointees under it, are eminently the province of this association, and that all legislation on the subject should be both inspired and dictated by the most prominent authority in the premises—the American Society of Civil Engineers.

April 18, 1875. C. SHALER SMITH.

Report II.

The undersigned differ from the views expressed in the foregoing report, and present the following as an expression of their own :

I.—They agree with the report, that it is desirable the American Society of Civil Engineers should publicly declare what it considers to be a standard bridge, anything below which is not to be deemed as a safe and durable construction. But they do not think it is desirable to go much into detail, as they believe it to be impossible to construct a specification that will meet all cases. Incompetent engineers cannot be prevented from building bad bridges by any specification however elaborate; they therefore are content with laying down general principles, leaving the application to others, and offer the following standard specification for bridges of iron and wood:

Spans—Feet.	Pounds.	Spans—Feet	Pounds.
100 and under.	100	300 to 400	60
100 to 200	80	Over 400	50
200 to 300	70	——	—

1. Every highway bridge shall be capable of carrying, in addition to its weight, a moving load per square foot of roadway and sidewalks as follows :

2. Every railroad bridge shall be capable of carrying on each track in addition to its own weight, 2 locomotives coupled, weighing 30 tons on drivers in space of 12 feet, and whose total weight, including loaded tenders, is 65 tons each; said locomotives to be followed by so many loaded coal cars weighing one ton per lineal foot, as will cover the remainder of the span.

3. Bridges shall be so proportioned that the above loads shall not strain any part of the material over one-fifth of its ultimate strength.

II.—The signers of the foregoing report, propose to cause future bridges to come up to the standard by a system of inspection, the inspectors to be passed by the Society before being appointed. The undersigned believe that in the present state of public opinion this is impracticable. If any inspectors are appointed, it will be by political influence, and the results will be worse than at present, as the inspection will be insufficient, and yet, to a great extent, relieve the owners of bad bridges from legal responsibility.

The undersigned consider that the most the Society can hope to do, is to provide means in case of the fall of a bridge, by which the responsibility of imperfect construction (if this was the cause of the accident) may be fixed on designers and builders, and iron manufacturers.

It is therefore recommended that the Society prepare and present to the state legislatures, a petition embodying the following data:

1. That the standard of the American Society of Civil Engineers shall be the legal standard, and in case it should be found that any bridge is of less strength than this, it shall be taken as *prima facie* evidence of neglect on the part of its owners.

2. That no bridge shall be opened for public traffic until a plan, giving the maximum loads it was designed to carry, the resulting strains, and the dimensions of all the parts, sworn to by the designers and makers, and attested by the signature of the proper officer representing the municipality or corporation by whom it is owned, be deposited in the archives of the Society, and that the principal pieces of iron in the bridge be stamped with name of maker, place of manufacture and date.

The result of this will be, that in case of the fall of a bridge, the responsibility can be directly and easily traced to the right party, which at present cannot be done, and the Society should willingly aid to such a purpose. This, it is recommended, should thus be done: the Society to appoint a committee—with compensation to be fixed by law—which, upon the call of the executive of any state, should visit and report upon any fallen bridge, care being taken that no parties interested in the construction of the bridge be upon the committee.

It is believed by the undersigned, that the knowledge all bridge builders would have that their misdeeds, if any, could, by this process, be traced home to themselves, would make them very careful in the future, and eliminate all failures of imperfect design or material.

As to the inspection of existing structures; if the society assumed the first duty, this would soon fall under its jurisdiction, and if it would volunteer the duty—in case any plan was deposited obviously unsafe—to protest against it, that also would be well; such would have prevented the fall of the Dixon bridge, and the lamentable loss of life and limbs there occurring.

THOS. C. CLARKE.
Feb. 1st, 1875. JULIUS W. ADAMS.

REPORT III.

The undersigned differ from the views expressed in the foregoing reports, and respectfully present the following :

1. The members of the committee agree that it is meet and proper the American Society of Civil Engineers should determine the standard strength for all bridges to be built in this country, and they further agree in the main, what this standard should be. The differences in opinions grow out of the methods for incorporating this standard in the every-day practice of the country. Two general modes present themselves for so doing; the one legislative and compulsory, and the other looking forward to directing public sentiment to right conclusions by a thorough dissemination of the adopted standard.

2. The undersigned advocate the latter method as the true policy of the Society, believing that any attempt to influence the enactment of laws that would be so far-reaching as the ones proposed, is impracticable, if not contrary to the genius of the Society itself. They further believe that when once public sentiment is aroused by the publicity which should be given to the adopted standard, it will compel the passage of laws covering the question.

3. The undersigned therefore suggest that the report to be accepted be simply one covering a standard strength for all bridges, in as general terms as possible, with a recommendation that such standard be widely disseminated by circular and the public prints, and that copies be distributed among the legislative bodies of the several states.

The following standard, culled from the foregoing reports, is proposed for adoption :

4. For highway bridges, as submitted in Report I.

5. For railroad bridges:*—the structure shall be at least capable of carrying on each track, in addition to its own weight, 2 locomotives coupled, weighing 30 tons on drivers in space of 12 feet, and whose total load, including tender, is 65 tons each. Said locomotives to be followed by as many loaded coal cars, weighing one ton per lineal foot, as will cover the remainder of the span.

Bridges to be so proportioned that the above described loads shall not strain the several parts in excess of one-fifth or one-sixth of the ultimate strength. In determining the strains produced by the above standard, it is to be understood that the chord system is to be computed for a uniform loading, while the web strains must be based upon the irregularly distributed or concentrated loads produced by the above described train, in its passage from one end to the other.

The following table represents the uniform distributed moving load for different spans :

* Being the same as submitted in Report II, page 129.

Span or Panel.	Pounds per Lineal Foot of Track.	Span or Panel.	Pounds per Lineal Foot of Track.
12 feet.	5,250	75 feet.	3,000
15 feet.	5,250	100 feet.	2,750
20 feet.	5,000	150 feet.	2,500
25 feet.	4,500	175 feet.	2,400
30 feet.	4,200	200 feet.	2,300
50 feet.	3,250	200 to 300 feet.	2,250

The extreme panel weight for all spans is obtained by using the standard weight per foot for short spans.

6. Under the standard loading, as expressed in above table, materials should not be strained in excess of what is submitted in Report I.

All of which is respectfully submitted.

ALFRED P. BOLLER.
March 1st, 1875. CHAS. MACDONALD.

REPORT IV.

While agreeing in many important particulars with the report of the Chairman of this Committee, the undersigned holds the views expressed by some of the other members regarding the expediency of compulsory legislation on the subject. It is believed that the opinions of the Society as a body, advanced for its interest and benefit and that of those who should choose to be governed by them, would have more weight and influence than though the Society should assert itself as a competent authority upon bridge construction. If this Society adopts a well defined standard of strength for bridges, it is believed that the public generally will wish to conform to it, and engineers even who are not members will be glad to avail themselves of the united opinion of so many of the profession.

There seems to be a unanimity of opinion among the members of the Committee as to what constitutes the ordinary load upon a railway bridge, and but a slight difference of opinion as to its amount.

From an examination of the weights carried upon many of the principal railways in the United States, it is found that the heaviest engines weigh about 2,830 pounds per foot; and that three, and sometimes four, are coupled. The heaviest weight on one pair of drivers is from 21,000 to 24,000 pounds, and the weight on all the drivers, generally not exceeding 12 feet wheel-base, is from 72,000 to 84,000 pounds. The heaviest trains may be assumed to weigh 2,250 pounds to the running foot, exclusive of the engines. As the coupling of more than two engines is mainly upon snow roads, it is not believed they should be included in a general rule for proportioning bridges, but should be classed among those exceptional cases for which a general provision cannot be made.

In view of the above, it is believed that all railway bridges should be proportioned for a rolling load of 3,000 pounds to the foot for the total engine length, and for 2,250 pounds to the foot for the remainder of the bridge; that bracing on each system should be proportioned to sustain 84,000 pounds on any 12 feet of track, and that any point on the track should sustain 24,000 pounds.

It is not believed that the system of expressing the loads that a bridge should carry, by so much per foot with a varying amount for each length of span, is the best; but if such a standard is to be adopted, the table in the report of the Chairman is believed to be the best of those given, although it is somewhat below the loads actually carried by many roads in this country.

The floor-beams of railway bridges should be proportioned for not less than the following loads:

Spaces.	Pounds.	Spaces.	Pounds.
4 feet apart, or less.	28,000	12 feet apart, or less.	42,000
6 feet apart, or less.	31,500	15 feet apart, or less.	45,000
8 feet apart, or less.	35,000	More than 15 feet apart	{ 3,000
10 feet apart, or less.	38,500		{ to the foot.

The following table is offered as embracing the foregoing loads when reduced to so much per lineal foot:

For intermediate lengths of span the proportional number of pounds per foot should be taken.

These loads do not include the extraordinary weights that are sometimes drawn over railways in this country; such as heavy pieces of machinery, blocks of stone, or a locomotive of different gauge on a truck car, nor more than

Span or Panel.	Pounds per Lineal Foot of Track.	Span or Panel.	Pounds per Lineal Foot of Track.
12 feet and under.	7,000	50 feet.	3,000
15 feet.	6,000	100 feet.	2,800
20 feet.	4,800	200 feet.	2,600
25 feet.	4,000	300 feet.	2,500
30 feet.	3,600	400 feet.	2,450
40 feet.	3,200	500 or over.	2,400

two engines coupled. These are exceptional cases which can be provided for when they may be expected to occur, and the weight can ordinarily be distributed so to cover a sufficient length of track as not to exceed the loads above given.

For the effect of wind, the maximum strain is believed to be about 40 pounds per square foot horizontal, and about 20 pounds per square foot vertical.

For highway bridges, the following table is offered as a substitute for that given in the report of the Chairman for the three classes of bridges named :

Spans. (Intermediate lengths in proportion.)	Pounds per Square Foot.		
	A.	B.	C.
100 feet and under.	100	75	60
200 feet.	80	60	50
300 feet.	70	50	50
400 feet.	60	50	50
500 feet and over.	50	50	50

The floor-beams and flooring should be of sufficient strength to sustain the following loads on four wheels :—Class A, 24—B, 16—and C, 8 tons respectively. These do not include the extraordinary loads sometimes taken over highways. They are exceptional cases and the weight can generally be divided.

With regard to the factors of safety to be used, it is believed that a less factor is required for the permanent and unchanging dead load, than for the vibrating and uncertain live load, which may, by accident, be increased beyond the limit for which it was computed. This, together with the fact that a larger factor for the dead load gives no additional strength to the bracing near the middle of the span, but only at the ends, leads to the following substitution for the factor offered in the report of the Chairman.

For wrought-iron and steel in both compression and extension—for the dead load including snow, $\frac{1}{3}$—for the live load including wind, $\frac{1}{4}$ the ultimate strength.

For cast-iron in compression only, and for lengths of not more than 20 diameters—for the dead load $\frac{1}{4}$, and for the live load, $\frac{1}{10}$ the ultimate strength. For large masses, as in arches, a factor of $\frac{1}{4}$ the ultimate strength may be adopted.

Bridges should be tested with the maximum loads which they are intended to sustain A less load would seem to be of but little use, and a much greater one might unnecessarily strain the structure. The load should be applied gradually, and the moment any undue deflection or crippling is observed, or the slightest diminution in the transverse section of any bar is occasioned, the load should be immediately removed and never repeated. If no actual rupture occurs, the bridge will probably be safe with 0.4 of the test applied. The acceptance of all bridges, after being constructed with proper proportions for the material used, should be subject to such a practical test.

THEODORE G. ELLIS.

April 20th, 1875.

AN EGYPTIAN RAILWAY. — A great railway in Egypt, from Cairo to Khartoum, is progressing rapidly ; it is proposed to extend it westward to Darfur. Plans have been prepared for a line from Khartoum to the frontier of Abyssinia, the acquisition of that country by the Egyptian Government being regarded as only a question of time.

—*Engineering.*

THE "DIRECT PROCESS" IN IRON MANUFACTURE.

By THOMAS S. BLAIR, Pittsburgh, Pa.

Transactions of American Institute of Mining Engineers.

I FEEL a certain sense of responsibility in bringing before you the subject of the direct process in iron manufacture. I am aware that, in such a body as I have the honor of addressing, there are few who are not already so well informed upon its past history that it would be a weariness to them to listen to anything else than an account of practical success. Yet, to claim that success involves so much that, if I do not make good my claim, I deservedly expose myself to severe criticism.

The whole literature of the art, so far as it relates to the direct process, is, up to this time, but a history of failure. It is safe to say that more money, time, and talent have been fruitlessly spent in the pursuit of this object than in all the other unsuccessful efforts in the whole line of iron metallurgy. A distinguished authority in patent law has remarked that "the invention records of the United States and of foreign countries are filled with the waifs and abandoned relics of these abortive struggles."

Dr. Percy, whose great work may be taken as an epitome of all that was worth mention, whether useful or curious in pig-iron metallurgy, up to the date of its publication (1864), after giving elaborate accounts of various attempts at the direct process, condenses his own opinion of all that had been then effected, into a brief but summary comment upon a pamphlet of one of the sanguine inventors who had said: "It is evident that the present mode of working iron ores, whether rich or poor, is not the most rational or economic one, although almost the only one in general use. They convert iron already malleable into cast iron, to be reconverted at much labor and cost, into malleable iron again."

To this Dr. Percy rejoins: "These questions are extremely obvious. They have been repeatedly proposed before, but never yet satisfactorily answered." Elsewhere he speaks of Chenot (who came so near success that the jury of the French Exposition of 1855 thought he had attained it, awarding to him one of the great gold medals; and Le Play pronounced his invention "The greatest metallurgical discovery of the age") as "poor Chenot," and ridicules the claims set up for him.

Gruner in his "Steel and its manufacture," 1867, translated by Lenox Smith, 1872, says: "Several metallurgists have thought that instead of smelting ores in a blast-furnace, it would be better to simply reduce them to the condition of soft or carburized sponge. They hoped to obtain purer products and consume less fuel by operating at a lower temperature. They were completely deceived. When the sponges are made, instead of cast-iron we have blooms of less purity, since they contain, besides the usual cinder, the earthy substances in the ore. And if the sponges are melted in crucibles instead of forging them directly in the form of blooms, we shall have a homogeneous product, but it will be iron or crude steel of inferior quality, unless the iron sponge undergoes fining like pig-metal. In the direct methods whose object is the abolition of blast-furnaces, the addition of carbon mixed with the ore cannot be avoided; and it is this which destroys all profit in the processes invented by Chenot in France, Renton in America, Gurlt in Germany," etc.

Bauerman who comes later than Percy (1868), gives but slight attention to the direct process. Speaking of the various processes for the direct production of wrought iron from the ore, he says: "As these methods are only applicable to the treatment of easily reducible ores and are essentially slow in work, giving only a small production from a plant of considerable extent, as compared with the open fire (Catalan forge), they have not as yet been found to possess sufficient advantages to be generally adopted on a large scale.

Crooks and Rohrig's work (1869), adapted from Professor Kerl's Metallurgy gives small encouragement. In the volume on iron they say, in their difinition of wrought iron: "It is usually

produced by the conversion of pig-iron, and, in rare cases is obtained direct from the ore." And again, under the caption, "Methods for Making Wrought Iron Direct from the Ore:" "At present this process is seldom used on account of its numerous disadvantages. It requires pure, rich, and easily fusible ores, and is performed in interrupted operations; much iron is scorified, the consumption of fuel is very large; and lastly the product is seldom uniform, and is mixed with slag, which can only be removed by repeated welding." After describing the Catalan forge, etc., they proceed as follows: "Gersdorff roasts sparry iron ore in reverberatory furnaces, and heats the roasted ore, together with coal, in crucibles. Clay heats ore and coal in a retort, and treats the reduced iron in a puddling furnace. Renton reduces the iron ores in vertical, slightly heated tubes, by means of carbonic oxyde gas, and forms the reduced iron into balls in a puddling furnace. Chenot submits the ores to a reducing roasting, to transform them into magnetic oxyde, which he finally crushes, and by means of an electro-magnetic apparatus, extracts the magnetic components; he then reduces the ore with carbonic oxyde gas, grinds the resulting spongy iron, mixes it with soda, presses into cylindrical shape, and at a suitable temperature draws it out into bars. Roger heats the iron, together with coal, in a rotating cylinder, and forms the balls in a puddling furnace. None of these methods seem to have met with any practical success."

In their volume on steel they say, under the heading, "Steel Direct from the Ore:" "Gurlt proposes to treat rich, pure iron ore in cupola furnaces by means of carbonizing and reducing gases, and to melt the resulting product in a gas reverberatory furnace, but this method has not proved successful when carried out on a large scale. By Chenot's method, rich, pure, ores are reduced in cupola furnaces by interstratified layers of charcoal; the resulting spongy products containing various amounts of carbon, are sorted and ground in mills, and the mass is pressed into cylinders and melted in crucibles, sometimes together with coal and a purifying and scorifying flux of manganese. This method has been tried in Belgium without economical success, and it does not permit the production of cast steel containing a fixed proportion of carbon.

The newest and most promising way of producing steel direct from the ore is Mr. Siemens' method with the regenerative gas furnace. This is the method described by Mr. Siemens before the Chemical Society of Great Britain, May 7th, 1868. The main feature is the vertical hoppers in which the ore was to be reduced, and the product dropped thence into the bath of an open-hearth furnace. (Further on we shall see that Mr. Siemens states that it has been abandoned.) Neither Kohn nor Fairbairn appear to have thought the subject worthy of serious notice.

Under the date of February 27th, 1869, we have the record of the opinion of a metallurgical chemist, known to you all as an eminent authority. I allude to Mr. Geo. J. Snelus. I quote from an English patent granted to him, of the date just mentioned: "In the ordinary process of making iron, the ore is reduced under such conditions that it immediately takes up carbon and is converted into cast iron. Several attempts have been made to produce wrought iron direct from the ore, but either owing to the process not being continuous, or its requiring too much time and fuel, or its inapplicability to the treatment of fine ore, and *the incomplete reduction* of the ore, none of these attempts have yet been successful in such a degree as to afford the means of making iron or steel so economically as can be done by first forming pig-iron in the blast-furnace."

On this side of the Atlantic, with one notable exception, the direct process received little attention in the literature of iron metallurgy. The exception I refer to is the report of Dr. T. Sterry Hunt, addressed to Sir W. Logan, Director of the Geological Survey of Canada, 1869. In this report the author says: "In accordance with the well-known fact that the reduction of oxyde of iron takes place at a temperature very much below that required for subsequent carburization and fusion, it has been shown that the charge of ore in the blast-furnace is converted to the metallic state some time before it descends to the zone in which melting takes place. It forms, when reduced, a spongy mass,

readily oxydized, which, by proper management, can be compressed and made to yield malleable iron, or by appropriate modes of treatment, may be converted into steel. This fact has been the starting-point of a great number of plans designed to obtain malleable iron and steel without the production of cast iron and the employment of the processes of puddling and cementation. This, it is true, is attained in the Catalan and blooming forges, *but the attention of many inventors has been, and still is, directed to the discovery* of simpler, or at least more economical, methods of obtaining similar results."

Dr. Hunt then proceeds to sketch all the direct process, in this country and abroad, worthy of mention, up to the date at which he wrote, pointing out in each case the difficulty or drawback developed in practical working. It is a brief but comprehensive history of the subject, and tells the same story in every case,—failure to reach any large results.

The British Iron and Steel Institute may certainly be taken as embodyidg the latest and most advanced ideas in everything that relates to iron metallurgy. At its meeting in London, March 19th, 1872, the discussion which arose respecting the Danks puddling furnace, brought out incidentally an expression of opinion on the direct process from some of its most eminent members. Mr. Edward Riley said: "As regarded making wrought iron direct from the ore, he believed there was certainly very little hope of that being carried out practically or profitably. He thought no one could conceive any method more simple than the present process of throwing materials into the blast-furnace for the purpose of reducing them, and he was sure that all improvements in iron should commence with the pig-iron. They could make it in any quantity, and they ough to start there. He could not conceive of any other process of making iron cheaper."

Mr. Isaac Lowthian Bell "thought that a certain amount of disrespect had been shown," in a previous part of the discussion, "with regard to the blast-furnace, in speaking of it as a roundabout way of doing the work which was performed by it. There was no doubt that they combined the iron with the carbon or silicon in the smelting process, which had subsequently to be dispersed; but they must remember that the blast-furnace, at the same time, got rid of earthy impurities generally found associated with iron ores. He therefore quite agreed with Mr. Riley that, although it might be a roundabout way in the first instance, they could not conceive any means so simple for getting rid of a large amount of extraneous matter as blast-furnaces."

These views appear to have been acquiesced in by the members generally. At their meeting in April, 1873, Dr. C. W. Siemens read a paper which, from the distinguished position of its author, and the character of its reception by his associates, may reasonably be supposed to represent the condition at that date of the art of iron making in Great Britain, so far as relates to the direct process. After describing the various attempts made by him to bring the direct process into practice, and explaining the reasons which induced him to abandon them, one after the other, he uses these words: "These experiments convinced me that the successful application of reduced ores could not be accomplished through their conversion into spongy metal, and fully explained to me the want of success which has attended the previous efforts of Clay, Chenot, Yates, and others, to produce iron direct from the ore." He then describes a new method and apparatus wherein he begins by abandoning one of the cardinal features of a truly direct process, a feature pointed out by Dr. Hunt in the extract I have already quoted, viz., that the reduction of the oxyde of iron can be obtained at a heat much below that required for its consequent combustion and fusion. Dr. Siemens, despairing of realizing this feature, begins, in his new process, by fusing the oxyde.

Such, I think, may be called a fair statement of the literature of the subject up to the present time. Furthermore, its uniform and consistent record of failure is borne out by the facts. It would have been, for example, impossible for a metallurgist so intelligent and deservedly esteemed as Gruner is, to commit himself to the statements I have quoted, if, at the time he made them,

there had been in existence, as an article of manufacture on a large scale, a *true* iron sponge. He speaks of the "earthy substances" as causing "impurity," and says that the sponge when melted will, it is true, give a homogenous product, but of inferior quality, "unless the iron sponge undergoes fining like pig-metal." Had he been acquainted with iron sponge whose only "impurities" (in quantities sufficient to be objectionable) were silica and alumina, could he have fallen into the error of stating that the impurities could not be removed by the state of fusion, but only "when the iron sponge undergoes fining like pig-metal?"

So with his statement that the necessity of adding carbon in the direct process "destroys all profit" in it. Had he been acquainted, I say, with true iron sponge, and familiar with its manufacture into iron and steel, he would have recognized the fact that in iron sponge we have the least possible affinity between the earthy substances and the metal. And he undoubtedly would have been thus informed had such practice been known in the art.

But setting aside all these, I come down to the present hour and present place, and our own country, and I ask you here present, who are familiar with all the industries of the nation, whether you have knowledge of any direct process for the production alike of iron and steel, now carried on upon a working scale, as a successful rival of the ordinary indirect methods?"

When one considers that the immense results which must flow from the successful achievement of the direct process are understood by all scientific men, and have been by them so understood for years past, it seems like presumption to attempt to carry off a prize which all have hitherto either despaired of, or, seeking, have failed to win. It seems so plain, so easy, yet has still remained, as it were, just out of reach. There must be, one would say, some hidden but insuperable difficulty, else the problem had long since been solved. Consider for a moment how inviting a field it is. Nature provides us with the metal we want, chemically combined with oxygen, and mechanically mingled with other substances. Let us withdraw this oxygen from the iron only, leaving the rest as compounds, it alone being elementary. Now let us melt the product, so that the iron shall, simply by difference of gravity, be separated from the dross, and then poured into proper moulds. Here we have but two steps, each of great apparent simplicity—first, reduction; second, fusion. Such is the ideal, which by contrast makes the old system appear so crude, unscientific, and roundabout, that the term "direct" applied to the new method sounds like the promise of a great and beneficent revolution.

We know that carbon at a certain heat will dissociate the iron and the oxygen, yet leave the other mineral matter of the ore unreduced, giving metallic iron—wrought iron—as the result. We know further, that we have at command furnaces in which the product can be melted down in a bath of cast iron, and so treated that it shall result in ingots of any desired degree of carburation. We know that if the reduction of the ore can be effected the elements of cost in fuel, labor, etc., will make the product cheaper than pig-iron, and also that the melting process is less costly than puddling, whereas its product is of far greater value. Why is it, then, that while the whole iron industry of the world is struggling by small economies to realize a return upon its capital, this most plain, most prominent of all economies remains unpractised?

There has been a link missing—without it, all is naught. There has been no thorough, uniform, economical process of reduction. The missing link is true iron sponge. It is that which I come here to exhibit to-day; to tell you how it is obtained, and to show you that, by the means I shall describe, it is within the reach of all. Let me be your guide while we travel together, in thought, from the point at which I started to the final point of success. It shall not be the path I traveled. This time we will take the smoothest and shortest way.

We are in a chemical laboratory. We take a small porcelain tube and fill it with a mixture of pulverized peroxyde of iron and charcoal; next we seal the ends of the tube hermetically, then expose it to heat, by immersing it in a bath of brightly red-hot sand for a certain time (varying with the character

of the ore), then take it out, cool it, and, after cooling, break it open, and pour out the contents. Carefully separating and testing them, we find that we have obtained particles of metallic iron. Now what condition did we observe to get this result?

First. There was contact of the iron oxyde with carbon.

Second. There was isolation from the free oxygen of the atmosphere.

Third. There was the heat of bright redness.

Fourth. There was a certain duration of time.

Fifth. There was continued isolation from the air until cold.

Hence, we have established the fact that if a peroxyde of iron be brought into contact with a sufficient quantity of carbon, with perfect isolation from the atmosphere while exposed for a sufficient length of time to a sufficient heat, and then cooled down to a sufficient degree while still isolated from the air, the oxygen and the iron will be dissociated, the oxygen passing off in a gaseous form, leaving the iron behind. Now, chemistry supplies all the data for filling up with absolute figures the blanks in this statement, and we have in consequence a formula by which, if strictly carried out, we can achieve the first of our two great steps in the direct process—we can gain the metallic iron directly from the ore. Hence the chemistry of the operation is clear, and it becomes simply an engineering question how to meet all the necessary conditions, so as to conduct it on the large scale.

First, we investigate previous attempts, striving to detect what is defective, recognizing what is correct, and supplying what yet is wanting. Proceeding in our course of elimination we first reject all those methods in which it is sought to yoke the production of the iron sponge directly with a method of treating it; those, for example, which are meant to reduce the ore in one chamber and pass it as fast as reduced (or supposed to be reduced) into another chamber for after-treatment—welding, melting, etc. The operations cannot be made synchronous. One or the other must be disarranged in order to accommodate its fellow.

Confining ourselves, therefore, to the simple question of reduction, we finally give the preference, among the multitude of contrivances and appliances, to the vertical chamber, to be filled at top, and drawn at bottom, and working continuously. But in all these we discover one fatal defect; there is no adequate provision for the isolation of the material, either while under treatment, or cooling, or both.

We experiment ourselves, and despair of obtaining the desired result by any arrangement of valves, or slides, or the like contrivances. The dilemma is this: we want an apparatus that, as I have said, shall work continuously, and on a scale of considerable magnitude, taking in and discharging material at short intervals, yet always closed to the entrance of free oxygen. Or, otherwise stated, we must have a chamber so open at top and bottom that we can dump in a cartload of crude material above and draw out a cartload of finished product below, yet be all the time hermetically sealed against admission of air. Now this chamber—assuming that we have settled upon the plan of filling it with ore and solid carbonaceous matter, and heating them through its walls—must be surrounded by heat for a certain distance down and by a cooling medium below that, because we intend to reduce and then cool down. Well, we find that our difficulty as regards the keeping out of air at top, takes care of itself. The solid oxygen and solid carbon, down in the zone of reduction, are combining as carbonic oxyde, and, by virtue of their great expansion, forcing their way upward and out so that they arrest every particle of free oxygen before it can penetrate downward. As to the bottom, however, we have not this resource, and must find another. We get it by giving to our chamber such proportions that there shall always be above the place of egress a column of material, so cool itself as to be proof against the influence of oxygen, and of such a height as to form a packing, which shall seal up all that material above it which has not yet reached the safe degree of cooling.

By this device, which, surely, is as simple as anything in metallurgical engineering, our dilemma is answered. We are now operating, in regular practice, at Glenwood, cylinders of three feet

internal diameter, and forty feet in height, whch are open tubes, so far as relates to the taking in and discharging of their contents, but as relates to access of air in their working zones, are sealed retorts; the seal above being the ingoing material itself and the gases percolating upward through it; and the seal below, the material which, by cooling, has become indifferent to exposure. For the first time, then, in the history of attempts at the direct process we have at our command complete isolation, yet continuous working.

Let us next take up the question of imparting and maintaining the necessary heat. Here at once another difficulty confronts us. We must work upon a scale of considerable magnitude, and our reducing chambers must, therefore, be of considerable area. But their contents are very poor conductors of heat, and a little experience will convince us of the impracticability of getting an evenly-distributed temperature by conduction from the outside through a mass of, say, three feet diameter. Now, we must have uniformity of temperature to get uniformity of result, and the system we have adopted obliges us to impart the heat by conduction. We could conduct it, we will say, through three inches of the materials, in time enough to answer all practical purposes, but not through three feet.

Let us see, therefore, if we cannot bring every particle of the material within three inches of a sufficiently heated surface. Thus stated, you will probably guess at the solution of the problem. It is this: When charging your material into your cylinder, cause it to pass between heated surfaces in streams whose greatest distance, in any part, from a sufficiently heated surface, shall not exceed your limit of three inches.

This, you will readily perceive, may be done in many ways. Let me describe to you one of the arrangements which I employ. It accomplishes very economically the purpose just explained, and performs another function which I will refer to directly.

In the top or mouth of the reducing cylinder, I suspend an inner cylinder or thimble of cast iron, with walls, say one inch thick, and having an outside diameter of twenty-eight inches.

Now, the reducing cylinder has an inside diameter of thirty-six inches; hence there is left an open space or annulus between the two of four inches across.

I charge my materials into this annulus only, so that all have to pass downward through it, and none can be more distant than two inches from the heated surface, either of the cylinder or of the thimble. I make the thimble long enough—say six feet—to insure that all the materials shall have acquired the temperature desired before they descend below the annulus.

This "initial heating," as I call it, establishes one of the primary conditions with which we started out—the imparting of the necessary degree of heat—the only duty required of that portion of the heating chamber which surrounds the cylinders below the level of the bottom of the thimble being to prevent the escape of the heat thus imparted. You will observe that this device completely meets the whole difficulty as to the conduction of the heat, so that—whatever the diameter of the reducing cylinder—it is only a question of what diameter and length you will give the thimble, in order to impart your materials the temperature you wish.

We have now got thus far. Our reducing furnace shall consist of one or more cylinders (adopting the cylinder as the preferable form of chamber), which shall be heated externally for a certain distance from the top down, then cooled the rest of the distance downward to the base, excepting the room required at bottom for raising the telescopic sleeve for the discharge of material.

At its top is the thimble for initial heating.

Let us now revert to our original statement of the conditions to be met, and see if we have fulfilled them.

First. We provide the contact of iron ore and carbonaceous matter by mingling them before discharging into our cylinder.

Second. We isolate these materials from free oxygen while in the zone of reduction.

Third. We conduct the required degree of heat through the mass.

Fourth. Our apparatus enables us to hold it under treatment for any length of time desired.

Fifth. We have continued the isolation until the product was too cool to be oxydized on exposure to the air.

Thus we have realized, upon a working scale commensurate with the requirements of the art, the laboratory experiment of the sealed tube, and the manufacture of iron sponge becomes as simple as any of the ordinary operations in the art of iron making.

From the general principles above laid down, it will be easy to plan a good working reducing furnace; but there are a number of details, both of construction and management, which I think may interest you.

I have already alluded to the thimble arrangement for the "initial heating," as having another recommendation beyond its convenient form. What I referred to is this: When the carbon dissociates the oxygen from the iron, carbonic oxyde is formed, and this, rising as I have said, passes outward by way of the interior of the thimble, as furnishing a line of less resistance than the annulus, packed as the latter is with the ingoing materials. As it ascends through the thimble it is met by the air, which, in virtue of its greater weight (being colder), and from the tendency to transfusion in gaseous bodies, descends into the thimble, and a perfect combustion of the carbonic oxyde is kept up.

Thus the carbon, which had served as a chemical agent in the reduction of the ore, is made to do duty once more, as a fuel.

Speaking of fuel, I would say that my method of heating the cylinders is to place the portion of them to be heated in a chamber of brick, which is supported on iron pillars; thus leaving the cooling zone accessible below. This chamber is heated by letting into it streams of gas at different levels, with an air inlet adjacent to each inlet of gas. All, of course, are arranged so as to have the gas supply under convenient control. Aside from the economy of gaseous, as compared with solid fuel, it is incomparably easier to keep a chamber such as this at a uniform temperature with gas than to heat it by burning coal or wood on grates.

While on this subject of fuel, I may say that I am tired of the ordinary form of gas-producer. It is certainly a clumsy affair.

I hope to have something interesting to say, upon a future occasion, as to a better form throughout. Meantime I would suggest to others who find the clinkering to be as much of an annoyance as I do, to try—as I shall soon—in the present form of producer, a water-tox all round, as high up as clinkers borm, and water-bars like those somemes used under boilers.

I not only introduce the gas into the heating chamber, but also carry a pipe into and project it downward nearly to the bottom of the thimble. By this means, whenever the gases developed in the cylinders, as before explained, do not suffice to keep the heat of the interior of the thimble up to the point desired, I turn on other gas enough to make up the deficiency.

Thus I secure perfect control of the heat of the thimble, and make sure that the material in the annulus will always be hot enough to be ready for dropping when a charge is drawn from below. In this way the output of the furnace is limited to but one consideration, to wit: what duration of exposure to a red heat is necessary to perfect the conversion. The amount of fuel required for heating is about one-third of a ton of iron in the sponge turned out. Any description of fuel commonly used in gas producers will answer. As to the cooling, the reducing cylinders underneath the heating chamber are prolonged simply in wrought iron of one-fourth inch thickness, and each is surrounded by a jacket, which is kept full of water continually changed. The wrought iron cylinder ends about eighteen inches above the floor, and a sleeve, working telescope fashion, closes the remainder of the connection when let fully down. By raising the sleeve more or less, as required, the material gushes out underneath, and as it does so the whole column of material in the cylinder descends, leaving a space at the top of the annulus, which is immediately filled up with fresh material.

I do not find that the size of the ore makes any practical difference, whether it is, say, two inches through, or any smaller. This fact has been observed in experimental work heretofore, but I have never seen, nor been able to frame for myself, any explanation that is quite satisfactory. I suggest it as an interest-

ing subject for our fellow-members of the chemical profession.

It has been stated in the books that the sesquioxyde of iron in the process of reduction first becomes magnetic oxyde, then protoxyde, then metallic. This appears to be demonstrated by the fact which may often (if not always) be observed in pieces too large to be "done through" (as the workmen phrase it), in the time during which they were under treatment.

If the size of the piece is large, say four inches, and the core is still quite raw, but the outside completely reduced, the concentric layer next to the core will be protoxyde, the next magnetic oxyde, and the next the iron sponge. Not that these layers are distinctly defined, but merge into each other at the points of contact. But if the size is kept within the limit named, there is usually no distinction to be observed, and the pieces, if anything near raw at the core, will usually show signs of protoxyde on the surface.

I must, however, qualify my remark as to the comparative time required for the reduction of pieces of different sizes. I did not mean to include ore in fine particles. This does appear to be more rapidly reduced than that which is coarser, but as it is cheaper to break the ore only to a moderately small size, and the fine powder is hence an insignificant fraction, I have not observed it closely in this particular.

With respect to the time required for treatment, it varies according to two sets of conditions.

The first is that of chemical composition. The sesquioxydes are more easily reduced than the magnetic, and the latter than the protoxydes. Hence, hasty reasoners, who might argue that because the sesquioxyde had to pass through the stages of magnetic and protoxyde before becoming metallic, it must, therefore, be the hardest to reduce, would find themselves in direct opposition to the fact. The explanation, I suppose, is this: Where the oxygen most abounds, reduction is easiest to commence, and once on the move, the operation proceeds rapidly.

The second set of conditions are those of mechanical structure. The massive materials are, as one would naturally suppose, harder to operate upon than those which are loose and open. The brown hematites are capital subjects for the reducing furnace. As soon as they reach a red heat, the water of combination is driven off, leaving an open, sponge like structure, and being also sesquioxydes, we have both the chemical and mechanical conditions for speedy reduction. The compact hematites, such as the Iron Mountain ore of Missouri, and the red specular of Lake Superior, though sesquioxydes, have no combined water, and are of a dense structure. In consequence they require a much longer treatment. The magnetic oxydes, such as those of Lake Champlain and the iron sands of the St. Lawrence, being both very compact, and leaner in oxygen, require a longer time still than the compact hematites; while the protoxydes, when in such a shape as, for example, the dense tap cinder from the puddling furnace, are extremely obstinate under treatment.

Among the curiosities of the reduction of iron oxydes, is the fact that the intensity of action bears but slight relation, within certain limitations, to the degree of heat employed. This is a fact noted by Mr. I. Lowthian Bell, in his experiments with the blast furnace. It suffices for our present purposes to state, as relates to it, that there would be no particular acceleration of the process gained by pushing the heat to a degree that involves danger of welding the material together while under treatment.

But I am able to announce to you another very important fact, and one not to be found in the books, namely, that at the temperature of reduction—say a fairly bright red heat, and with carbon alone as the reducing agent—no carbon whatever is taken up by the iron. I think it sufficiently indicates the state of the art of iron-sponge making as it has been hitherto, when I tell you that I asked this question direct of one of the most distinguished and most practical of the foreign authors I have already quoted, and the answer was that, to the best of his knowledge, the point had never been settled. Now, if you will consider for a moment the immense importance of this question—the question whether your product is to be wrought iron alone —a product which you can employ as iron or carburize with precision to the

temper desired, or whether it is to become an unsettled and uncertain carbide of iron, to be sampled and analyzed, every lot, before using, and from which carbon must be removed if wrought iron is to be made from it; when I say, you consider the magnitude of this question and the fact that neither the man of science nor the practical manufacturer had any answer for it, you will agree with me that the art had not yet made much progress.

But at all events, the question is now set at rest. I have had frequent analyses made of iron sponge, produced from various descriptions of ore, and in no case has combined carbon been found. The iron sponge, sensitive as it is to many chemical reactions, only takes up carbon (when presented unaccompanied by an accelerating agent) as other wrought iron does, to wit: at the recognized heat of comentation, a heat far higher than we need to (or ought to) employ in the reducing furnace.

With respect to the carbonaceous matter used as the reducing agent, I would state that, in regular practice, we have, up to the present time, made use of charcoal. We have tried both coke and anthracite, but merely in an experimental way. We have not been prepared to remove the sulphur from either, and—having so many other things to get into working order—have preferred to run no risks in this particular. Our experiments have been conclusive, however, as to the reducing power of these substances, and we shall, early in the spring, take measures to use coke from washed coal. We have experimented with a Bradford separator, and find that the fine "slack" of the Pittsburgh coal can be so freed from sulphur that even, if none were driven off in coking, and the whole of it absorbed by the iron in the reducing cylinder, there would not be over 0.08 per cent. in the iron. For the country east of the Alleghenies, the anthracite culm should furnish an exceedingly cheap reducing agent. I am informed that there is no difficulty in removing the sulphur by treatment with steam charged with alkaline vapors, and at moderate cost. I have not yet had any practical experience, however, in this matter.

The estimate of quantity required per ton of iron produced is very easily made. For brevity's sake we will consider only the sesquioxydes, as they require the largest ratio of carbon. They carry 70 per cent. of iron to 30 per cent. of oxygen. Now, every 30 parts, by weight, of oxygen take up 22½ parts of carbon, so that we employ 22½ parts of carbon for every 70 parts of iron, or 32.14 parts of carbon to the 100 of iron; in round numbers, one-third ton of carbon to the ton of iron in the sponge. It may occur to you that this is the theoretical quantity, and that in practice it must require more. But such is not the case—at least to any appreciable extent. No carbon is used in the reducing cylinder except what is taken up by the chemical operation referred to above. None of the other oxydes of which the ore is composed are reduced, and there is no free oxygen present to consume any carbon. Whatever excess, beyond the amount absolutely required, we may mix in with the ore, to secure a sufficiency throughout the mass, is regained at the bottom of the cylinder.

I would now ask your attention to the fact that, in my statements respecting reduction, I have hitherto confined myself to the case of reduction by carbon only. You are aware, however, that there are certain substances, such as cyanogen, hydrogen, etc., which, when present with carbon, exert a singular power in accelerating its combination with iron. Some of these substances, as, for example, hydrogen, are also in themselves powerful reducing agents. You will see at once how the employment of these may vary the results. The hydrocarbons, for example, will produce reduction at a lower temperature or with great saving of time, but will yield an irregularly carburized sponge. The field is too large to enter upon here, and must be passed over with this brief notice, to be reverted to, however, for a moment when I come to speak of the second branch of the direct process, viz., fusion.

There is another feature of the reducing operation which has been remarked, upon by some accurate observers, such as Mr. I. Lowthian Bell, but which appears to have been unsuspected by the generality of those who have attempted to make iron-sponge. It is this, that the

resistance of the oxygen against its dissociation from the iron increases in inverse ratio to the quantity remaining. Thus, to get out fifty per cent. of it, for instance, is a very easy and a very short operation; to get out the next twenty-five per cent. may perhaps not take much longer additional time than to abstract the first fifty per cent.; but the refractory quality in the oxygen keeps rapidly rising until it becomes practically almost a matter of impossibility to get out the last remnant of it. Ignorance of this simple law has kept many a sanguine inventor pursuing an *ignis fatuus*, and at its door must be laid the corpse of many a once cherished, but now lifeless "process."

From it arises the talk, sometimes so freely indulged in, about iron-sponge as a well-known article, quite at command if wanted.

As a curiosity in this line, let me quote from an English patent of 1870, taken out by a practical manufacturer, a manager of steel works: "The reduction of the ore to the condition of spongy metallic iron is a matter of comparatively little difficulty, and may be effected in various ways," etc.

These so-called iron-sponges carrying, say ten per cent. of the iron as protoxide, are not the materials with which to obtain a victory over the old, well-established, indirect process. They will give too poor an account in yield, however used, and they are especially objectionable for open-hearth practice. Dr. Siemens covers the whole ground in a few words, in his American patent of April 11th, 1871: "The metallic oxide corrodes the banks of the metal bath."

Let us turn now to the second step in the direct process, the fusion of the iron-sponge. I will pass by all other methods of treatment and confine myself to this, the most important. The open-hearth gas-furnace enables us to produce a homogenous product cast into ingots. We will not stop here to discuss the various definitions of "steel" as distinguished from "iron." For present purposes we will adopt the popular conception, and apply the term steel to what a blacksmith would call steel, that is, whatever will "take a temper," and iron shall mean what the blacksmith would call iron, that is, what will stand the same heat and weld the same way as that which he has always called iron, and which will not take a temper.

These ingots of iron or steel (according to the ratio of carbon contained) are produced by melting wrought iron in cast iron. Here then is an operation for which the sponge is especially adapted.

It is more fusible than any other form of wrought iron, and its mineral portion will be separated by the act of fusion without any special treatment whatever.

I had no other idea than to use the "Siemens" regenerative gas-furnace (that being the one invariably employed heretofore in open-hearth practice) until I came to arrange for a license, when I was informed by the agents in this country of Dr. Siemens, that my license must contain the stipulation that I could only employ in the furnace such materials as he (Dr. Siemens) would permit me to use; and my iron-sponge was not embraced in the list. After repeated efforts I found it impossible to shake his determination that his furnace should not be employed for any other direct process than his own. I was, therefore, obliged to look elsewhere, and, happily, found what I sought in the gas-furnace of Mr. H. Frank, of Pittsburg.

This furnace works on a system of "continuous regeneration," the waste gases passing continuously in one direction outward, and the air and gas supply passing continuously in one direction inward. I regret that the length of this paper compels me to omit a detailed description of this most satisfactory furnace. It gives all the heat that can be used (the endurance of the structure being the limit), and works with the greatest steadiness. All clogging of the regenerators by tar and soot is avoided by the simple expedient of alternating the currents of gas and air so that the air is made to pass through the chamber where the gas had previously been, thus burning out all those deposits, while the gas finds a clear passage in the other chamber where the air had been flowing. It is only neccessary to make this alternation between heats, so that, from the time of charging until the cast is made, the only manipulation called for is the adjustment of the inlet-valves for gas and air, and of the damper of the stack.

Our present practice at Glenwood is to take the iron-sponge and press it, while cold, into blooms of six inches diameter and about twelve to eighteen inches in length. A specimen of these is exhibited here. The pressing is performed by hydraulic machinery, and the force exerted is about 30,000 pounds to the square inch, or about 900,000 on the bloom. Thus prepared, we charge them into an auxiliary heating-furnace, where they are brought to a bright-red heat, and then thrown into the bath of the melting-furnace. We use no other form of wrought iron whatever. Otherwise there is nothing peculiar in our operations, and everything goes on just as if we were melting ordinary blooms, except that the fusion is much more rapid. We have no difficulty whatever with the lining of the furnace, owing to the small amount of protoxide left in the sponge, there being decidedly less than is usually found in puddle-bar. It is here that the perfection of the reduction tells.

We have operated hitherto with ores so rich—Iron Mountain of Missouri, and Red Specular of Lake Superior—that we have no excess of slag. On the contrary, we generally find it expedient to throw in a little cinder from a previous cast. When using ores which carry so much earthy matter that the slag would be in excess, we shall "bleed" it away, to the extent desired, from a cinder-notch which we have provided in the wall of the furnace.

I propose to do away with pig-iron, at first in part, finally altogether. There are two ways of doing this, both of which I shall practice long enough to determine which seems preferable, and hope to have the pleasure, on some future occasion, of reporting the results to you.

In the first method I avail myself of the system of rapid carburization practiced in "case-hardening" and in the melting of wrought iron in crucibles, viz., the employment of an accelerating agent, such as cyanogen, along with common carbonaceous material. Mixing one or more of these agents with charcoal-dust, and the resultant mixture again with sponge before pressing, I have a bloom which holds the carburizing materials in intimate contact with the particles of iron, and it is a question to be developed by experience what amount of carbon can be imparted to the iron up to the time of its fusion.

In the second method I take up Gurlt's idea of the carburization of sponge by hydrocarbon vapors, and apply it to my reducing furnace in this way: I have tapped a gas-pipe into one of the cylinders, so as to furnish an inlet by which I can force gas into and among the contents of the cylinder. This inlet is placed just above the cooler, so that the gas will enter when the material is yet hot, but has passed below the zone of reduction. I generate gas from benzine, in an apparatus placed at such a distance from the building as to be safe, and under pressure sufficient to overcome the resistance in the cylinder. I shall thus get the carburizing action without any other extra expenditure of fuel than the small amount required for generating the benzine gas. The apparatus is now just ready to go into operation, and I expect to impart such a quantity of carbon to the sponge as to render it readily fusible without the aid of a bath of cast iron.

I consider it a very desirable step in the perfecting of the direct process, that we should dispense with cast iron in the open hearth for two cogent reasons: first, because we have now turned the tables, and wrought iron is cheaper than pig; and second, because the less pig we use the better the quality of the product. Dr. Siemens, in the paper from which I have already quoted, puts this matter in a very clear light. Speaking of the desirability of a direct process, as regards the question of quality, and referring to one of Mr. I. Lowthian Bell's diagrams of a blast-furnace, he says:

"It shows that the reduction of the metallic oxydes to spongy iron is accomplished within the first twenty feet in their descent in the furnace, and at a comparatively low temperature. This upper zone is followed by one where the limestone is decomposed and the carbonization of the spongy metal is commenced. Between this second zone and the zone of fusion in the boshes of the furnace, one of great magnitude intervenes, where apparently no other change is effected than an increase of temperature of (the) spongy metal, but where in reality a very powerful reducing action

is accomplished of substances which had much better not be joined to the iron. It is well known that almost all the phosphorus contained in the ironstone and the coke is here incorporated with the spongy iron. The silica is reduced to silicon, and, together with arsenic and other bases which may be present, combines with the iron. The final action in the blast-furnace only consists in fusing those reduced substances and forming the slags which envelop and protect the fused metal."

On the other hand, as I have already explained, the low temperature at which the reduction of the iron oxyde takes place in the direct process gives no opportunity for reduction of the other oxydes accompanying it. Hence, though the mechanical union remains, there is no chemical affinity, and as we, in our second step, produce the fusion under conditions which do not allow time for the reduction of the other substances, we get away our iron uncontaminated.

Here, again, I have to regret that time and your endurance do not allow me to do more than refer to some exceptions to this, in the case of sulphur and phosphorus. As to the former, it is perhaps unnecessary for me to explain how the difficulty can be overcome. As to the latter, I will say, speaking from absolute experience, that no difficulty arises where the phosphorus exists—as in the Lake Champlain ores—in the condition of phosphate of lime. With respect to other phosphorus-bearing ores, I hope to make a special report to you, when I can enter into details which are inadmissible here, and after I have more extended experience. I must also defer anything beyond a mere casual reference to titanium, which gives no trouble in the direct process.

Dismissing these interesting topics, I close my explanatory statements, trusting that nothing further is needed to satisfy you that you have now presented to you a perfectly practical and thoroughly direct process for obtaining the ingot of cast steel or homogeneous iron.

Little need be said as to the value of this product. Open-hearth practice has already established the fact that steel fit for all purposes short of edge tools can be produced (even when using the system of melting wrought into cast iron), and that the homogeneous metal is the type of all perfection in wrought iron. With respect to the results which will follow the introduction of the direct process into the field of iron metallurgy, I do not venture any prediction as to how speedy or how slow may be the revolution. Some time must elapse, during which the old system will regulate the market price, while the new system will (for those employing it) regulate the cost. But with such data as I will now very briefly call your attention to, it is easy to see that the old system must either be greatly cheapened, or it must, sooner or later, be overgrown by the new.

The direct process demands so much smaller an amount of fuel that the proper plan for realizing the most profitable results in practicing it will be to go to the mines, and there produce the sponge at least; in many cases the ingot also. The extreme simplicity of the plant required, and the ease with which the process can be conducted on a small scale, if desirable, also point to the mine as the proper locality for the works, up to, as I say, the sponge always, the ingot often.

Take, now, such a locality, where ore of 50 per cent. metallic iron is worth $4 per ton, and charcoal is worth 6 cents per bushel, We have:

2 tons ore at $4	$8 00
40 bushels charcoal, at 6 cts	2 40
Gas producing fuel (wood) say	1 00
Wages, say	3 00
1 ton iron in sponge	$14 40

Let us add $5.60 per ton for transportation to a manufacturing centre, making the cost of the sponge, say $20, delivered. Add $2 per ton for cold pressing.

One ton of ingots will cost about as follows:

⅔ ton cold-pressed blooms, $22	$16 50
15 per cent. waste on the same	2 48
¼ ton Bessemer pig, at $45	11 25
7½ per cent. waste on the same	84
Wages, per ton	5 00
Maintenance of furnace, &c.	2 50
Spiegeleisen, 1/10th ton, at $70 per ton	3 50
¾ ton fuel for producers, at $5 per ton	3 75
Cost of 2,240 lb. ingots	$45 82

Assuming that we shall be able to substitute carburized sponge for the Bessemer pig, we reduce this to about $38.50.

The figures must be varied to suit every different locality, and in those where ore is a high-priced commodity and fuel cheap, there will not be as great a difference in favor of the direct process as where those conditions are reversed; but there will always be enough to give it an advantage that must tell eventually.

Finally, there is one aspect, at least, of this branch of the subject that must be gratifying to all. I refer to the humanitarian view. The word "puddling" finds no place in the direct process. No such exhausting, overtaxing labor is demanded in any of its operations, and, as it is the truly scientific method of iron metallurgy, so does it, in common with all true science, point to the ultimate reconcilement of capital and labor.

I desire, before closing, to take this opportunity to acknowledge my indebtedness to my associate and colaborer, Mr. Morrison Foster, of Pittsburgh, whose assistance, from the first inception of my experiments up to the present time, has been of the greatest value to me.

PROF. EGLESTON desired to know how complete the reduction was, how much oxygen remained in the sponge, and how the impurities common to iron ores were eliminated. He said that in the paper just read there were some severe remarks on the crude condition of iron metallurgy, especially the blast-furnace process. He desired to say that the blame did not lie at the doors of scientific metallurgists in this country. It must be remembered that most of the experiments abroad had government aid for their experiments, and government furnaces at their disposal to practice on. For the last thirteen or fourteen years he had endeavored to make experiments on blast-furnace gases, but had never been able to overcome the prejudice of furnacemen to having holes made in the stack of the furnace. Prof. Egleston spoke of some investigations made by Director Jüngst, of Gleiwitz, on the temperatures at which ores begin to lose oxygen in the blast-furnace, and the temperatures at which reduction is complete. The temperature of incipient reduction is stated by Jungst to be much lower than is generally supposed. Regarding the elimination of sulphur from coal by washing and coking, Prof. Egleston spoke of the works of the Orleans Railway, at Aubin, in the South of France, which he had studied, where a refuse coal containing 12 per cent. of ash and iron pyrites in large quantities, in lumps from the size of a hickory-nut to fine grains, was worked so as to contain only 3 per cent. of ash and 0.5 per cent of sulphur.

MR. BLAIR: We find 95 to 98 per cent. of the iron reduced. The impurities in the ore, as silica, alumina, lime, etc., are all contained in the sponge; but when the sponge is introduced into the bath of molten pig metal, the earthy ingredients melt and rise to the surface in the form of slag. In rich ores the amount of slag is not enough to cover the molten metal, and slag is added as such. In poor ores the amount of slag may be too large, and provision is made in the cinder-notch for tapping it off. The height of this notch is raised or lowered by means of fire-brick, according to the height of metal in the furnace.

MR. F. FIRMSTONE asked Mr. Blair what became of the phosphorus in the ore in his process.

MR. BLAIR: We have made steel in crucibles from sponge made from Lake Champlain ores, which contained a large amount of apatite, and found no phosphorus in the steel. The case might be different where the phosphorus was combined with the iron in the ore.

MR. RAYMOND remarked that it might make considerable difference if the phosphorus was combined with manganese in the ore. He had heard of a case recently, in which Bessemer pig was said to have been made from an ore containing 0.58 per cent. of phosphorus, and at the same time considerable manganese. It may be that phosphate of manganese is reduced with great difficuly, or that manganese will tend to carry off the phosphorus in the slag. He would like to ask Mr. Blair what became of the carbonic oxyde escaping from his cylinders.

MR. BLAIR: It is burned within the thimble to carbonic acid, and exerts no injurious influence on the workmen. He

had had a few quite serious cases of poisoning with carbonic oxyde arising from his gas-producers, and had invariably found ammonia (spirits of hartshorn), applied to the nostrils, a prompt and efficient redemy. When nausea is produced by inhaling carbonic oxyde, a few drops of the aromatic spirits of ammonia give relief.

Prof. B. Silliman said that the question of the influence of manganese in smelting ores containing phosphorus was an interesting, and, to a considerable extent, an unexplored field. He had in mind a case where a spiegeleisen, containing 11 per cent. of manganese, and 0.1 per cent. of phosphorus, was said to be made from a spathic ore containing but 0.5 per cent. manganese, and 0.6 per cent. phosphorus. He thought that this could only be explained by the addition of manganese in some form to the charge, and in this connection the unexpectedly small amount of phosphorus in the spiegel was suggestive.

Mr. E. B. Coxe: The subject of poisoning by carbonic oxyde is one of such great importance that I think that all possible publicity should be given to the antidotal effect of ammonia mentioned by Mr. Blair. I think it very probable that the "white damp" of the mines is carbonic oxyde, and its fatal effects are well known to miners.

Mr. E. C. Pechin: I have listened to the very able paper read by Mr. Blair with melancholy pleasure. As a humanitarian I am delighted, as a pig-metal manufacturer I am in the depths of despair. I am placed in a position which must appeal powerfully to your sympathies. A few weeks since I was blown up by physical force—to-day I am blown away by scientific investigation. All my beautiful plans for new furnaces must be stowed away with the inscription, "What might have been if it had'nt been for Blair." In behalf of the pig-iron makers of the United States, I appeal to Mr. Blair to follow the example of Dr. Siemens, to surround his process with such restrictions, and to charge such excessive royalties, that we may for this generation, at least, rather die by slow combustion than meet a violent and hasty death by carbonic oxyde.

Mr. Blair reminded Mr. Pechin that he had used the expression that the old process will be *overgrown*, not *overthrown*.

Dr. Hunt expressed his pleasure at the results obtained by Mr. Blair, whose works near Pittsburg he had an opportunity of visiting in November last. He felt a great interest in the question of iron-sponge, from the fact that he had been the friend of Adrian Chenot, who had, in 1855, works in operation on a considerable scale at Clichy-la-Garenne, near Paris, and had assisted him in some of his experiments just before his sudden and accidental death at the end of that year. Chenot died with many of his plans unrealized, leaving behind him no one fully competent to carry on his work. Dr. Hunt testified that, notwithstanding the difficulties encountered, Chenot did succeed, at least with the readily reducible and porus Spanish ores, in obtaining a complete reduction, as the regular daily manufacture from the sponge of cast steel, which he had personally overlooked and followed, sufficiently showed. The apparatus of Chenot was essentially that of Mr. Blair, but there were practical difficulties in the way of heating the column which have been overcome by the latter by means of his simple and ingenious initial heater, in which the gas wasted from the top of Chenot's furnace performs the work of heating the ore in the upper part of the cylinder; while by the happy device of using a mixture of charcoal in powder, instead of in lumps, the difficulty of preserving the reduced ore from the influence of the air below is resolved. By these additions to the furnace of Chenot, Blair has continued and perfected his work.

But the ready production of iron-sponge was but one part of the problem; its utilization was still more difficult. The conversion of the sponge into cast steel by cementation with oil, and fusion in a crucible, as practised at Clichy by Chenot, was, at best, but a slow and troublesome method; and the attempt to weld the sponge into blooms, as tried at Clichy, and afterwards practiced at Baracaldo, in Spain, was an expedient not easy of execution, and applicable only to very pure ores. The work of Chenot, of Gurlt, and of others, in making iron-sponge, was in vain; the time had not yet come for its economic utili-

zation, nor was it until the brothers Martin, with the aid of the Siemens gas-furnace, succeeded in producing steel on a large scale in the open hearth from the fusion of soft iron with cast iron, that the true use of the sponge, as a substitute for puddled iron, was found.

This new process again turned the attention of inventors to the production of iron-sponge, and three or four years since a reduction-furnace, erected for the purpose at Westport, on Lake Champlain, succeeded in producing sponge which, at the Bay State Works, at South Boston, gave in the Siemens-Martin process a soft steel, with excellent results. This reduction-furnace, which the speaker had examined, seemed, however, but indifferently fitted for its work, and was soon abandoned. The simple, cheap, and efficient apparatus of Chenot has, in the hands of Mr. Blair, received such improvements as made it, in the speaker's opinion, admirably fitted for the purpose of reducing iron ores to sponge. He regretted exceedingly that the beautiful and ingenious reduction-furnace constructed by Mr. Edward Cooper, at Trenton, which many of the members of the Institute had an opportunity of inspecting in October last, was not already in operation, so that we might be enabled to judge of its practical efficiency. For the rest, the speaker entertained no doubt that the economic production of iron-sponge, and its utilization in the open hearth, in accordance with the Siemens-Martin plan, was destined to be one of the great metallurgical problems of the future. One of the most important advantages of this process is the fact pointed out by Mr. Blair, that the mechanical impurities of the reduced ore are readily and completely eliminated by the process of dissolving it in a bath of molten metal. The iron is reduced to the metallic state without the reduction of phosphorus and silicon, and the compounds of these are not attacked by the metallic bath, which takes up the reduced iron as mercury takes up the precious metals in the process of amalgamation.

EFFECTS OF STRESS ON INDUCTIVE MAGNETISM IN SOFT IRON.

By Prof. Sir WILLIAM THOMSON, F. R. S.

Proceedings of the Royal Society.

1. At the last ordinary meeting of the Royal Society (May 27), after fully describing experiments by which I had found certain remarkable effects of stress on inductive and retained magnetism in steel and soft iron, I briefly referred to seeming anomalies presented by soft iron which had much perplexed me since the 23d of December. Differences presented by the different specimens of soft iron wire which I tried complicated the question very much; but one of them, the softest of all, a wire specially made by Messrs. Richard Johnson & Nephew, of Manchester, for this investigation, through the kindness of Mr. William H. Johnson, gave a result standing clearly out from the general confusion, and pointing the way to further experiments, by which, within the fortnight which has intervened since my former communication, I have arrived at a complete explanation of all that had formerly seemed anomalous. These experiments have been performed in the Physical Laboratory of the University of Glasgow by Mr. Andrew Gray and Mr. Thomas Gray, according to instructions which, in my absence, I have sent from day to day by post and telegraph.

2. The guiding result (described near the end of my former paper, and referred to in the last paragraph but one of the Abstract in Proceedings of the Royal Society for May 27) was, that the softest wire, tried with weights on and off repeatedly, after it had been magnetized in either direction by making the current, in the positive or negative direction, and stopping it, gave effects on the ballistic galvanometer which proved a shaking out of residual magnetism by the first two or three ons and offs, and a gradual settlement into a condition in

which the effect of "on" was an *augmentation*, and the effect of "off" a diminution, of the inductive magnetization due to the vertical component of the earth's magnetizing force. When a fresh piece of the same wire was put into the apparatus and tested with weights on and off it gave this same effect. If the wire had been turned upper end down and tried again in the course of any of the experiments, still this same effect would have been shown. It seemed perfectly clear that in these experiments there was no other efficient dipolar quality of the apparatus by which the positive throw of the ballistic galvanometer could be given by putting on the weight, and the negative throw by taking it off, than the vertical component of the earth's magnetic force.

3. Yet I did not consider that I had *explained* the result by the terrestrial influence, because, for *all* the specimens of steel and soft iron, the effect of weights on had been uniformly to *diminish*, and of weights off to *augment* the magnetism when the magnetizing current was kept flowing. And I was, moreover, perplexed by the magnitude of the result—the effects of weights on and off shown by the very soft iron wire, under only the feeble magnetizing influence of the earth, being many times (from three times to nine or ten times) as great as the effects which the same weights on and off produced in the same wires when under vastly greater magnetizing forces of the currents through the helix.

4. But by reducing the strength of the magnetizing current gradually, it was clear that the small positive effect of the "on" with the positive current flowing and the small negative effect with the negative current must be gradually brought to approximate more and more nearly to the large positive effect of the "on" when there is no current at all. Immediately after my former communication I therefore arranged to have experiments made with different measured strengths of current, feebler and feebler, until the law of the continuity thus pointed out should be ascertained; and so speedily arrived at the following astonishing conclusions:

5. (1) When the magnetizing force does not exceed a certain critical value the alternate effects of *pull* and *relaxation* are respectively to augment and diminish the induced magnetization.

(2) When the magnetizing force exceeds the critical value the effects are —pull diminishes, relaxation augments, the induced magnetization.

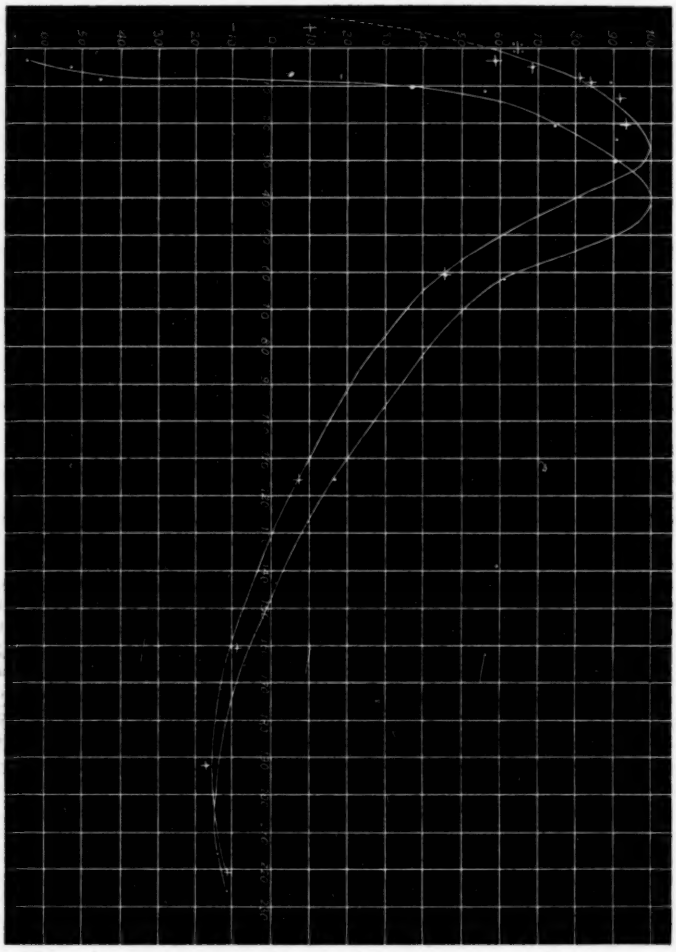
(3) The critical value of the magnetizing force for the annealed Johnson soft iron wire, with 14 lbs. on and off, is about 17 or 18, if (for a moment) we take as unity the vertical component of the terrestrial magnetic force at Glasgow.

(4) The maximum positive effect of the pull on the inductive magnetism is obtained when the magnetizing force is about 4.

(5) The positive effect of the pull when the magnetizing force is 3 is about eight or nine times the amount of the negative effect when the magnetizing force is 25.

6. The actual results of the experiments which proved these conclusions are exhibited graphically in the accompanying diagram. The hotizontal scale (abscissas) shows the numbers of divisions of the scale of the steady current galvanometer (called for brevity the "battery-galvanometer") used to measure the strengths of the current through the helix. The scale of ordinates shows the numbers of divisions of the scale of the ballistic galvanometer by which the sudden changes of the magnetism of the wire produced by 14 lbs. "on" and 14 lbs. "off" were measured. The ordinates are drawn in the positive direction when the effect of "on" is to increase and of "off" to diminish the magnetism. The simple round spots show the results of observations with currents in the direction called negative (being those which gave negative deflections of the battery-galvanometer). The spots in the centre of signs (+) show results obtained with currents in the direction called positive. The star (*) at the position 64 on the line of ordinates through the zero of abscissas shows the mean effect of many ons and offs with no current flowing—that is to say, when the sole magnetizing force is the vertical component of the earth's magnetic force. The curves are drawn as smoothly as may be by hand, one of them to pass as nearly as it can (without intolerable roughness) through all the crossed (plus) dots and

the star at 64, the other through all the plain dots. The latter curve cuts the line of abscissas at 8, this being the result (telegraphed to me this evening) of special experiments made to-day for the purpose of finding accurately the amount of the negative current which, by neutralizing the vertical force of the earth or the wire, gives an accurate zero effect for the "off" and "on." The dotted prolongation of the curve through the plus's, to cut the line of abscissas on its



negative side, is ideal, and is inserted to illustrate the relation of this curve to the other. By the two curves cutting the line of abscissas at + 8 and — 8, we see that 8 is the strength of the current, measured on the scale of the battery-galvanometer, which gives a magnetic force in the axis of the helix equal to the vertical component of the terrestrial magnetic force.

7. Next a series of experiments to test the inductive effects of repeatedly making the current always in one direction, and stopping it, with the weight of 14

lbs. always on, and again with the weight off, and this with various degrees of current, feebler than those used in the earlier experiments. The results with all the different intensities of magnetizing force thus applied were the same in kind as that which I found on the 23d of December, operating with a much stronger magnetizing force on the first soft iron wire tried; that is to say (contrarily to what I had found in the steel wires), *the change of magnetization produced by repeated applications and annulings of the magnetizing force of the helix was greater with the weight off than on.*

[*Note on Diagram, added July* 2, 1875.—A continuation of the experiments with higher and higher magnetizing powers, since the communication of this paper, disproves the negative minimum indicated by the curves on the diagram, and proves an asymptotic approach to a value approximately — 12, of ordinates for infinitely great positive values of the abscissas.]

THE HYDRAULIC DOUBLE FLOAT.

By HENRY L. ABBOT, Major of Engineers, Brevet Brigadier General.

Written for VAN NOSTRAND'S MAGAZINE.

In the August number of this Magazine appeared an article by Prof. S. W. Robinson, on River Gauging and the Double Float.

As he refers therein to some observations made with the latter upon the Mississippi Delta Survey, I will very briefly correct a few misapprehensions into which he has fallen.

Some are of little importance; as, for instance, when he states that the double float was used upon "the Mississippi, previous to the Delta Survey, by Mr. Chas. Ellet. This is an entire mistake, as Mr. Ellet's first trial of them was made after they had come into regular daily use by the parties of the Delta Survey.

Prof. Robinson argues that the current meter in some form must be superior to the double float, because it has been adopted more generally by hydraulic engineers — and he establishes the fact of more general use by citing the names of many engineers whose observations were made with it. If the dates of most of the measurements had been given, it would have been at once apparent, that, although suggested long ago, the double float as now used is really the more modern instrument of the two. The flint lock musket has been employed far more in great battles than the modern breech loader, but its superiority is not established thereby.

It would seem that the fairer criterion of the merits of the two instruments, would be the practical results obtained from their use. Now, it is certain that, although many careful observers employed time and money, and displayed great scientific ability in endeavoring to discover the law of change in velocity from surface to bottom by the use of meters, they utterly failed to detect the form of the curve. Whereas, on the very first serious attempt with the double float, a law was revealed which has since received many confirmations, and which has greatly simplified the operation of the practical gauging of rivers by showing, first algebraically and afterward by actual trial, that the ratio of the mid depth to the mean velocity is practically constant, and is even unaffected by the wind. The scientific Engineer Corps of India has recognized the value of the modern form of the instrument, and is now extensively applying it in their operations upon the great rivers of that country. The latest and most accurate work in river gauging done in this country since the date of the Delta Survey—I refer to the unpublished material of the Connecticut River Survey conducted by General Ellis, which will appear in the forthcoming report of the Chief of Engineers U. S. Army—establishes the facts that the double float and meter, properly used, give sensibly the same result; and that the fundamental law respecting flowing

water announced in the Delta Report for the Mississippi River, is true also for the Connecticut. So far then as the useful record of the two classes of instruments is concerned, the double float is no whit behind.

Prof. Robinson fails to touch upon the great practical objection to meters—namely the uncertainty which attends the determination of the coefficient for translating their revolutions into feet per second. So long as it remains impossible to exactly reproduce the same identical conditions in deducing this coefficient, which are to affect the observations themselves, so long will there be grounds for doubt and uncertainty in this vitally important point. Therefore, without disputing the value of the instrument for certain kinds of work, its superiority to the double float for detecting slight changes in velocity may well be doubted.

Without following Prof. Robinson in his application of the higher mathematics to the theoretical solution of the problem of the mutual influence of the several parts of the double float upon each other; I would like to suggest one or two ideas.

He treats the problem upon the assumption that the curve of velocity from the surface to the bottom of a river, is unvarying. Now all observations show that a continual irregular pulsation is going on; and that the mean curve is only to be deduced from many observations. Hence the different parts of the double float are acted upon by varying forces; and thus their *masses* cannot be neglected, as he has done, in treating the subject. In other words, there is a continual gain or loss of living force in the several parts which will prevent the large and heavy sub-float from being affected as his equations indicate.

Whatever may be the value of these equations for other rivers, we are not left in doubt as to their entire inapplicability to the Mississippi River, at least as he has applied them. This truth does not rest upon any theory, but upon a fact observed again and again, and recorded in the note books of the Survey. To make this clear a few words are necessary.

No matter how deep the river, we found that, if the lower float touched bottom, the sudden check in velocity gave an unmistakable oscillation to the surface float. The points of crossing the transit lines, two hundred feet apart, were both fixed accurately by triangulation; and the telescope of one or other of the observers was kept on the little flag during the whole of the critical period of its motion. The exact soundings in the vicinity, and the daily gauge records, rendered it possible to know precisely the depth of water in every part of the path of the float. Now it was sometimes the case that a float would diverge a little, laterally, into water too shoal for its length of line—and in such cases it at once revealed the fact by the bobbing of the flag—which was duly noted in the record book. We have, therefore, certain knowledge that in many cases, and probably in all, our deep floats preserved the depths at which they are reported. Prof. Robinson's diagrams and imputations, therefore, evidently do not apply to the work of the Delta Survey.

I have only one more remark to add; Prof. Robinson lays much stress on our neglect to reduce the size of the connecting cord—and suggests, in its place, a wire filament, a hundredth of an inch in diameter. The importance of using cords of the minimum size was perfectly appreciated; and, on the only occasion during the Survey when it was practicable to use a fine wire to advantage, viz. on the Little Falls Feeder of the C. and O. Canal, reported on page 252 of the report, such a wire was actually used—probably for the first time in the history of the double float. The reason why, in our deep measurements on the Mississippi, we used so large a cord, was because it was found by experience to be necessary. The cord, in raising the float, had at times to sustain severe strains, amounting to fifty or more pounds; and smaller cords had not the requisite strength. The operation of gauging the Mississippi in flood was a struggle—not a delicate laboratory task. The whirl of the waters, which six oars vigorously plied in a light skiff could hardly stem; the rushing drift-logs, which at the peril of life must be avoided; the passing steamers that often seemed to enjoy interrupting our work; and, lastly, the importance of multiply-

ing observations as rapidly as possible, in order to keep the finger firmly upon the pulse of the great river—all compelled the use of apparatus which would endure rough handling without breaking.

In conclusion, I would express the hope that nothing in the foregoing communication may seem to imply any desire on my part to undervalue the interesting article of Prof. Robinson, which opens a new subject for analytical investigation. I only wish to show that, in applying his formulæ to the Mississippi Delta Survey observations, he was not informed as to all the circumstances in the case.

ON THE THEORY OF VENTILATION—AN ATTEMPT TO ESTABLISH A POSITIVE BASIS FOR THE CALCULATION OF THE AMOUNT OF FRESH AIR REQUIRED FOR AN INHABITED AIR SPACE.

By Surgeon-Major F. DE CHAUMONT, M. D.

From the Proceedings of the Royal Society.

The question of ventilation, and the amount of fresh air required to keep an inhabited air-space in a sweet and healthy condition, has been much discussed of late years, and very fully treated of by various writers; but there was a good deal of vagueness and want of precision in the manner of treatment previous to the Report of the Committee on Metropolitan Workhouse Infirmaries in 1867. In a paper in the 'Lancet' in 1866 I attempted to show that a more scientific method might be employed, and suggested some formulæ, which we quoted by Dr. Parkes in a paper appended to the Report above mentioned. Professor Donkin also investigated the question there, and in a short but exhaustive paper showed that, general diffusion in an air-space being admitted, the same amount of air was required to ventilate it, whatever its size might be. In another paper, published in the 'Edinburgh Medical Journal' in May 1867, I went into the subject with the view of pointing out that we might, with existing data, establish a basis, which should be both scientific and practical, for estimating the amount of air required; and I adduced some results to show that the evidence of the senses might be employed (if used with proper care and precautions) as the ground-work of a scale, and gave a short table of the amounts of respiratory impurity (estimated as CO_2) which corresponded to certain conditions noted as affecting the sense of smell. This paper attracted the attention of General Morin, who made it the text of a short article in the Journal of the Conservatoire des Arts et Métires during last year. Since the publication of my paper in 1867 I have accumulated more data; and the number of observations being now sufficient to give at least a fair approximation to the truth, I beg to call attention to the results.

It is generally admitted that it is organic matter, either suspended or in the form of vapor, that is the poison in air rendered impure by the products of respiration. It is also admitted that it is the same substance that gives the disagreeable sensation described as "closeness" in an ill-ventilated air-space. Although the nature of the organic matter may vary to a certain extent, it will be allowed that a condition of good ventilation may be established if we dilute the air sufficiently with fresh air, so that the amount of organic matter shall not vary *sensibly* from that of the external air. Unfortunately all the methods devised for the determination of organic matter in air are both difficult and unsatisfactory, so much so that they are almost practically impossible in a ventilation inquiry. Observations, however, as far as they have gone, seems to show that the amount of organic impurity bears a fairly regular proportion to the amount of carbonic acid evolved by the inhabitant in an air-space; and as the

latter can be easily and certainly determined, we may take it as a *measure* of the condition of the air-space. This being accepted, and general diffusion being admitted, we can easily calculate the amount of fresh air required to bring down the CO_2 to some fixed *standard*, adopting as a datum the ascertained average amount of CO_2 evolved by an adult in a given time. If, now, we adopt as our *standard* the point at which there is no sensible difference between the air of an inhabited space and the external air, and agree that this shall be determined by the effects on the sense of *smell*, our next step is to *ascertain* from experiment what is the average amount of CO_2 in such an air-space, from which we can then calculate the amount of air required to keep it in that condition. The sense of smell is very quickly dulled, so that, in order to keep it acute, each air-space to be examined ought to be entered directly from the open air. For this reason I have not included in the present paper any of the observations made in prisons, as it is almost impossible, from their construction, to enter the cells directly from the open air. All the results, therefore, have been obtained in buildings where this could be done, viz. barracks and hospitals, and several were examined.

The plan followed in all was to take the observations chiefly at night, when the rooms or wards were occupied, and when fires and lights (except the lamp or candle used for the observation) were out. In this way all disturbing sources of CO_2 were avoided, except in the occasional rare instances of a man smoking in bed or the like. On first entering the room from the outer air the sensation was noted and recorded just as it occurred to the observer, such terms as "fresh," "fair," "not close," "close," "very close, "extremely close," &c. being employed.* Most of these notes were made by myself; but a good many were also made by my assistants, Sergt. (now Lieut.) Sylvester in the earlier, and Sergt. H. Turner in the latter experiments. The air was then collected (generally in two jars or bottles, for controlling experiments), and set aside with lime-water for subsequent analysis, and the temperatures of the wet- and dry-bulb thermometers noted. About the same time samples of the external air were also taken, and the thermometers read. In this way any unintentional bias in the record of sensations was avoided, and this source of fallacy fairly well eliminated.

In some of the earlier observations the CO_2 in the external air was not observed as constantly in connection with the internal observations, partly because the importance of this was not so clearly perceived then, and partly from want of apparatus, the jars used being very bulky and not easy of carriage. It might therefore be argued that the *combination-weights* of the earlier experiments should be less in calculating the averages. I do not think, however, that this would amount to any sensible difference in the result, as the external CO_2 ratios adopted from single experiments accord fairly with the mean ratio of the outer air*. In each case the CO_2 has been corrected for *temperature*, but not *for barometric pressure*, and in some cases the reading of the barometer was not taken; the difference, however, would not exceed on an average 1 per cent. The vapor and humidity were calculated from Glaisher's Tables.

Although the records of sensation are various in terms, I have thought that they might be advantageously reduced to *five* orders or classes, as follows:

No. 1. Including such expressions as "fresh," "fair," "not close," "no unpleasant smell," &c., indicating a condition giving no appreciably different sensation from the outer air.

No. 2. Including such expressions as "rather close," "a little close," "not very foul," "a little smell," &c., indicating the point at which organic matter begins to be appreciated by the sense of smell.

No. 3. "Close," indicating the point at which organic matter begins to be decidedly disagreeable to the sense of smell.

No. 4. "Very close," "bad," &c., indicating the point at which or-

* N. B. The terms used in the Tables are *exactly* those noted down at the time of observation.

* Mean ratio of the whole series .372; omitting those at Portsmouth Garrison Hospital, which were exceptionally low, 413.

ganic matter begins to be offensive and oppressive to the senses.

No. 5. "Extremely close," "very bad," &c., indicating the point at which the maximum point of differentiation by the senses is reached.

Where there was a slight smell of tobacco no change in the record was made; but where the smell of tobacco was strong, the observation was generally referred to the next order, both because the presence of the tobacco-smoke indicated slow change of atmosphere, and also because the sense of closeness must have been considerable to make itself felt along with the tobacco. Hence such a remark as "rather close," which properly belongs to No. 2, is referred to No. 3, "close," if accompanied with a strong smell of tobacco.

The total number of observations for the temperature, vapor, and humidity in the inhabited spaces amounts to 247*, and of carbonic-acid analyses to 473. Where the latter are in pairs they are linked by a bracket. In each case the external and internal observations and their differences are given, and the arithmetical means of all are taken. In the differences which represent the quantities due to respiratory impurity, the mean error, error of mean square, and probable error (both of a single measure and of the result) are calculated, and the limits shown between which the range would lie in each case. The values are also given as the reciprocals of the squares of mean error and of probable error of result, and their ratios to No. 1 as unity. The modulus is also calculated from the mean error and error of mean square, and the ratio of the two resuls thus obtained shown as another means of estimating the value of the series.

Analyses of the different Orders.

No. 1. "Fresh," &c.: a condition of atmosphere not *sensibly* different from the external air.

1. *Temperature.*—The experiments were made during both winter and summer, so that there is a good deal of variation in the external temperature, and the mean is some degrees above the mean annual temperature of this country (southern part of it), viz. 57°.47. The mean in the inhabited air-spaces is 62°.85, or 5°.38 higher. This is a moderate difference, and shows a good average temperature for dwelling-rooms. The maximum range is 10° (57°.89 to 67°.81), calculated from the error of mean square, the actual extremes being 77° and 53°.

2. *Vapor and Humidity.*—As the external temperature varied considerably, so also did the amount of vapor, the mean being 4.285, equal to about 80 per cent. of humidity. The internal observations showed a mean of 4.629, or 73 per cent. of humidity, being an excess of vapor of 0.344 of a grain, and a lowering of relative humidity equal to 7 per cent.

3. *Carbonic Acid.*—The mean external carbonic acid was 0.4168, a little above the usual amount. The mean in the inhabited air-spaces was 0.5998, or an excess of 0.1830, the mean error being 0.0910. The probable error of a single observation is 0 0831, so that the truth would lie between 0.2661 and 0.0999; whilst the probable error of the result is only 0.0078, the range being between 0.1908 and 0.1752; we are therefore entitled to say that the limit of impurity, imperceptible to the sense of smell, lies at or within 0.2000 volume of CO_2 per 1000 as a mean. From these data, then, we may lay down as conditions of *good* ventilation the following:

Temperature about 63° Fahrenheit.
Vapor shall not exceed 4.7 grains per cubic foot.
Carbonic acid shall not exceed the amount in the outer air by more than 0.2000 per 1000 volumes.

No. 2. "Rather close" &c.: a condition of atmosphere in which the organic matter begins to be appreciated by the senses.

1. *Temperature.*—In this series the external temperature (although still above the mean temperature of this climate) was rather lower than in the previous one, viz. 54°.85, whilst the internal observations gave a mean of 62°.85 (the same as in No. 1), or a difference of 8°.

2. *Vapor and Humidity.*—Although the temperature was the same as in No. 1, the amount of vapor in the inhabited air-spaces was greater, both actually and

* It has been thought unnecessary to give these in detail as taking up too much space, but the means are given at the end of the Table of Carbonic Acid.

relatively, the excess being 0.687 of a grain and the lowering of humidity being about 7.6 per cent.

3. *Carbonic Acid.*—The mean amount in the outer was 0.4110 per 1000 volumes, in the inhabited air-spaces 0.8004, or a mean difference (respiratory impurity) of 0.3894. The range for the probable error of result lies between 0.4057 and 0.3731.

We may therefore say that ventilation ceases to be *good* when the following conditions are present :

Vapor in the air exceeds 4.7 grains per cubic foot.
CO_2 in excess over outer air, ratio reaching 0.4000 per 1000 volumes.

No. 3. "Close" &c.: the point at which the organic matter begins to be decidedly disagreeable to the senses.

1. *Temperature.*—The temperature in this series was more near the mean of our climate, viz. 51°.28. The mean in the inhabited air-space was 64°.67, or a mean excess of 12°.91.

2. *Vapor and Humidity.*—The vapor in the outer air was 3.837, and in the inhabited air-space 4.909, a mean difference of 1.072 grain per cubic foot. The drying of the air amounted to a lowering of the humidity by 11.56 per cent.

3. *Carbonic Acid.*—The carbonic acid in the outer air was 0.3705 per 1000 volumes, rather below the average. In the inhabited air-spaces it was 1.0027, or a mean difference of 0.6332 due to respiratory impurity, the range for the probable error of result being between 0.647 and 0.617.

We may therefore say that ventilation begins to be decidedly *bad* when the following conditions are reached :

Vapor reaches 4.9 grains per cubic foot.
Carbonic acid in excess over outer air to the amount of 0.6000 per 1000 volumes.

No. 4. "Very close," &c. : the point at which the organic matter begins to be offensive and oppressive to the senses.

1. *Temperature.*—The mean external temperature was 51°.28, and the internal 65°.15, or a mean difference of 13°.87.

2. *Vapor and Humidity.*—The mean vapor in the outer air was 3.678 grains, and in the inhabited air-spaces 5.078, or a mean difference of 1.400 grain per cubic foot. This corresponds to a lowering of the humidity by 8°.58 per cent.

3. *Carbonic Acid.*—The mean amount in the outer air was 0.3903 per 1000 volumes, pretty near the usual average. In the inhabited air-spaces it was 1.2335, or a mean difference due to respiratory impurity of 0.8432, the range for probable error of result being between 0.8640 and 0.8224.

We may say that ventilation is *very bad* when :

Vapor reaches 5 grains per cubic foot.
Carbonic acid in excess over outer air reaches 0.8000 per 1000 volumes.

No. 5. "Extremely close," &c. : the maximum point of differentiation by the senses.

1. *Temperature.*—The temperature in the outer air was 51°.86, and in the inhabited air-spaces 65°.05, giving a mean difference of 13°.19.

2. *Vapor and Humidity.*—The mean vapor in the outer air was 3.875, and in the inhabited air-spaces 5.194, showing an excess of 1.319 grain, corresponding to a lowering of relative humidity of 9.88 per cent.

3. *Carbonic Acid.*—The mean amount in the outer air was 0.4001, or exactly the average amount. In the inhabited air-spaces it was 1.2818, showing an excess due to respiratory impurity of 0.8817 per 1000 volumes, the range for the probable error of result being between 0.9202 and 0.8432.

The extreme point of differentiation by the senses is thus reached when the following conditions are found :

Vapor 5.100 grains per cubic feet.
Carbonic acid in excess over the amount in the outer air beyond 0.8500 per 1000 volumes.

It will at once be seen that the figures in No. 5 differ but little from those in Mo. 4, and that the probable *limit of differentiation* by the senses is reached in No. 4. The number of recorded observations in No. 5 is also very few compara-

tively; and I think it would therefore be better to group the two together, as below.

Nos. 4 and 5 combined, being the probable limit of possible differentiation by the senses.

1. *Temperature.* — In the outer air 51°.43, in the inhabited air-spaces 65°.12, or a mean difference of 13°.69.

2. *Vapor and Humidity.*—The vapor in the outer air was 3.729, inside 5.108, or a mean difference of 1.379 grain, corresponding to a lowering of relative humidity of 8.92 per cent.

3. *Carbonic Acid.*—In the outer air 0.3928, in the inhabited air-spaces 1.2461, or a mean difference to respiratory impurity of 0.8533, the range for probable error of result being between 0.8717 and 0.8349.

We may therefore, I think, say that when the vapor* reaches 5.100 grains per cubic foot, and the CO_2 in excess 0.8000 volume per 1000, the maximum point of differentiation by the senses is reached.

By referring to the Tables it will be seen that there is a regular progression as we pass from one order to another. The following abstract shows this:

No.	Temperature.		Vapor.		Carbonic Acid.	
	In air-space.	Excess over outer air.	In air-space.	Excess over outer air.	In air-space.	Excess over outer air.
1	62.85	5.38	4.629	0.344	0.5999	0.1830
2	62.85	8.00	4.823	0.687	0.8004	0.3894
3	64.67	12.91	4.909	1.072	1.0027	0.6322
4	65.15	13.87	5.078	1.400	1.2335	0.8432
5	65.05	13.19	5.194	1.319	1.2818	0.8817

The progression is complete in the carbonic acid, although there are slight retrogressions in the temperature and vapor of No. 5. Taking the last two combined, we have

65°.12	13°69	5.108	1.379
	1.2461	0.8533	

We have now the progression complete throughout. Adopting *four* orders, then, we shall find the regularity of progression sufficiently note-worthy in the *vapor* and *carbonic acid*, the two products of respiration. It is less regular in the temperature, as might indeed be expected, from the varying condition of the external air.

TABLE OF DIFFERENCES OF TEMPERATURE, VAPOR, AND CO_2.

No.	Temperature.		Vapor.		Carbonic Acid.	
	Actual excess over outer air.	Progressive difference.	Actual excess over outer air.	Progressive difference.	Actual excess over outer air.	Progressive difference.
1	5.38	..	0.344	..	0.1830	
2	8.00	2.62	0.687	0.343	0.3894	0.2064
3	12.91	4.91	1.072	0.385	0.6322	0.2428
4 and 5 (combined).	13.69	0.78	1.379	0.307	0.8533	0.2211

* It is to be understood that the amounts of vapor stated in these cases are in reference to a mean temperature of about 63° F.

In each observation there is a culmination at No. 3, and a decline at the next

order. The average rates of progression (including the actual excess in No. 1) are :

Temperature.	Vapor.	Carbonic Acid.
3°.42	0.345	0.2133

Here the amount of vapor is exactly the actual excess in No. 1, and the amount of carbonic acid somewhat in excess; the mean, however, between this amount and the actual recorded excess in No. 1 is as follows :

Actual excess over outer air in No. 1...	0.1830
Mean of progressive increase, as above.	0.2133
Sum.....	2)0.3963
Mean.....	0.1982

This is sufficiently close to 0.2000 to furnish some additional reason for adopting this latter number as the limit of respiratory impurity admissible in *good ventilation.*

Values of the several series, considered relatively to each other.

The values are important as a guide to the more or less trustworthy character of the series. They have been calculated out in three ways :

1. As the reciprocal of the square of *mean error.*
2. As the reciprocal of the square of probable error of result.
3. As the ratio between the *modulus* calculated from the *mean error* and the *modulus* calculated from *the error of mean square of a single measure.*

The following Table gives the values from the first method, viz. as reciprocal of the square of mean error :

No.	Temperature.	Vapor.	Humidity.	Carbonic Acid.
1	0.0821	6.1300	0.0190	122.0000
2	0.0625	3.1300	0.0140	34.0000
3	0.0403	2.6500	0.0110	21.8000
4	0.0543	2.7700	0.0120	17.0000
5	0.0664	1.3700	0.0090	14.1000
4 & 5 combined	0.0610	2.2900	0.0010	16.5000

And the ratios, taking No. 1 as 1000, are :

No.	Temperature.	Vapor.	Humidity.	Carbonic Acid.
1	1000	1000	1000	1000
2	760	510	735	277
3	492	431	575	178
4	662	450	630	139
5	810	224	473	115
4 & 5 combined	745	374	526	135

Here we see that there is a diminution of value pretty regular up to No. 3, when there is a rise in No. 4 and No. 5 in the temperature, a rise in No. 4 and a fall in No 5 in the vapor and humidity, whilst the fall is progressive throughout in the carbonic acid.

In each case the result of the combination of 4 and 5 gives a number which takes its proper place after No 3, except in the temperature.

The following Table give the values according to the second method, viz. as reciprocal of the square of the probable error of the result :

No.	Temperature.	Vapor.	Humidity.	Carbonic Acid.
1	5.2716	293.93000	1.2656	16378.2000
2	4.1165	324.2300	0.5318	3750.4000
3	3.7470	281.3300	1.0966	4148.1000
4	3.7100	170.000	0.5439	2307.5000
5	1.5839	34.2770	0.1986	674.3000
4 & 5 combined	5.3171	195.8300	0.7708	2957.5100

And the ratios, taking No. 1 as 1000, are :

No.	Temperature.	Vapor.	Humidity.	Carbonic Acid.
1	1000	1000	1000	1000
2	781	1103*	420	229
3	711	957	867	253
4	704	578	432	141
5	302	117	157	41
4 & 5 combined	1008*	667	609	181

Here we see much the same order preserved, except that in two cases marked * (*Nos.* 4 *and* 5, *temperature, and No.* 2, *vapor*) the amounts exceed No. 1. It is also observable that in the vapor, humidity, and carbonic acid No. 3. is superior to No. 2. In every case the combined 4 and 5 series is superior to the two singly, being nearly their sum. In all the Tables it may be observed that the humidity is somewhat irregular in relation to the amount of vapor. This may be understood from the fact that it is a complex quantity, depending partly on the amount of vapor, and partly on the temperature.

If we now seek to get a general expression of the relative values of all the observations in each *order*, we may take the product of their values by the different methods.

TABLE SHOWING THE PRODUCTS OF THE VALUES OF EACH ORDER, CALCULATED FROM THE RECIPROCALS OF THE SQUARES OF MEAN ERRORS.

No. of Order.	Product.	Ratio.
1	1.1720	1000
2	0.0931	794
3	0.0256	218
4	0.0307	262
5	0.0115	98
4 & 5	0.0230	196

TABLE SHOWING THE SAME FROM PROBABLE ERROR OF RESULT.

No. of Order.	Product.	Ratio.
1	32139057	1000.00
2	2661995	83.00
3	4794655	149.00
4	791570	25.00
5	7254	0.23
4 & 5	2373680	74.00

Here we see a greater irregularity, No. 3 showing a superiority over No. 2, due probably to the greater number of individual observations in the former case.

Taking the mean of the ratios by the two methods, we have :

No.			No.		
1	=	1000	4	=	144
2	=	439	5	=	49
3	=	184	4 & 5	=	135

But the discrepancy in the ratios of the values from the probable error, where No. 3 exceeds No. 2, is due to the irregularity in the humidity column; and as this is not an independent quantity, but dependent on the temperature and vapor, we may legitimately omit it. We shall then have the products as follows :

VALUES FROM MEAN ERROR.

No.	Value.	Ratio.
1	61.40	1000
2	6.65	108
3	2.33	38
4	2.56	41
5	1.28	21
4 & 5	2.30	38

VALUES FROM PROBABLE ERROR OF RESULT.

No.	Value.	Ratio.
1	25382435	1000.00
2	5005632	197.00
3	4372692	172.00
4	1455360	57.00
5	36526	1.44
4 & 5	3079500	121.00

It will be seen that in the calculation from mean error there is a rise at No. 4 in both instances, *i. e.* with and without the humidity. There is a fall at No. 5, whilst the combined series 4 and 5 gives a result which follows naturally after No. 3. We may now reject Nos. 4 and 5 as separate orders, and consider them in combination, when we shall have the following relative values:

No.	From Mean Error.	From probable Error of Result.
1	1000	1000
2	108	197
3	38	172
4 & 5	38	121

And the mean of the two values will be:

No. 1.....1000	No. 3 108
No. 2..... 153	Nos. 4 & 5... 80

We have now a series of ratios which follow a regularly descending scale, very much in the order we might have expected *a priori*, seeing that the sense of smell is naturally less acute as the organic matter increases in amount. But it is of less consequence to determine the position of the higher orders in the scale, except as a measure of the general value of the observations throughout the inquiry, the really important point being the very great superiority of the first order, particularly as regards the carbonic acid. This is an additional argument for its adoption as the limit of admissible impurity in good ventilation.

The amount of fresh air necessary to keep the impurity down to the particular limit would be according to the following formula,

$$d = \frac{e}{q}$$

where d is the delivery of fresh air in cubic feet per head per hour, e the amount of carbonic acid expired per hour by one inmate, and q the limit of respiratory impurity taken as carbonic acid per cubic foot. If we take e to be the 0.6 of a cubic foot in a state of complete repose, such as during sleep, we are rather under Pettenkofer's estimate, but considerably above Angus Smith's. The following Table gives the amounts necessary for the three estimates:

No. of order.	Limit of respiratory impurity per cubic feet.	Cubic feet of air per head per hour calculated from		
		Angus Smith's estimate, $e = 0.450$.	Proposed estimate as adopted by Dr. Parkes, $e = 0.600$.	Pettenkofer's estimate, $e = 0.705$.
1	0.0001831	2460	3280	3850
2	0.0003894	1155	1540	1810
3	0.0006322	710	950	1115
4 & 5	0.0008533	530	700	825

I think that the general opinion is that Angus Smith's results give too low an estimate, and that 0.600 is really the lowest that can be with safety admitted.

The existing Army Regulations contemplate a delivery of 1200 cubic feet per head per hour in barracks; but practical inquiry has shown that this amount is generally fallen short of. The result is that the life of the soldier, at least during his sleeping-hours, is passed in a No. 3 air-space, or one in which the organic impurity is *decidedly disagreeable to the senses.* Previous to 1858 he did not even get this moderate amount of air; so that his life was spent in an air-space in which the organic matter was *offensive and oppressive to the senses.* If we adopt (as proposed already) 0.2000 per 1000 of CO_2 as the limit of impurity, then 3000 cubic feet per head per hour is the amount which must be delivered, on the supposition that $e=0.600$, or 3525 if $e=0.705$.

We may say, in conclusion, that the experimental data already quoted fairly justify the adoption of the following conditions:

Conditions as to the Standard of good Ventilation.

Temperature (dry bulb) 63° to 65° F.
" (wet bulb) 58° to 61° F.

N. B.—The temperature should never be very much below 60°, but it may be found difficult to prevent its rising in hot weather. In any case the difference between the two thermometers ought not to be less than 4°, and ought not to exceed 5°.

Vapor ought not to exceed 4.7 grains per cubic foot at a temperature of 63 F., or 5 grains at a temperature of 65° F.

Humidity (per cent.) ought not to exceed 73 to 75.

Carbonic Acid.—Respiratory impurity ought not to exceed 0.0002 per foot, or 0.2000 per 1000 volumes.

Taking the mean external air ratio at 0.4000 per 1000, this would give a mean internal air ratio of 0.6000 per 1000 volumes.

By considering separately the conditions found in barracks and in hospitals, or among healthy and among sick men, a point of some interest and importance seems to be indicated—namely, that more air is required for the latter than for the former to keep the air-space pure to the senses. This is due either to the greater quantity of organic matter or to a difference in its quality and nature. The following results are found from the data in the Tables:

	Barracks.	Hospitals.
Mean amount of carbonic acid per 1000 volumes as respiratory impurity found when the air was noted as "fresh,"&c., the impurity not being appreciable to the senses.....	0.196	0.157
Number of analyses in each group.....	75	38

Assuming the average carbonic acid per head to be 0.6 of a cubic foot, these amounts indicate a supply of air as follows:

	Barracks.	Hospitals.
Amount of air supplied per head per hour in cubic feet..	3062	3822

Stated in round numbers, therefore, we may say that while a barrack-room may be kept sweet with 3000 cubic feet, it will take 4000 to keep a hospital ward containing ordinary cases in the same condition. Much more would, of course, be required during times of epidemic or the like.

There is less regularity in the higher orders; but if the whole of the observations, other than No. 1, are taken together, we find a similar indication:

	Barracks.	Hospitals.
Mean amount of carbonic acid per 1000 volumes, as respiratory impurity, in all the observations, when the organic matter was appreciable by the senses	0.601	0.580

Calculating the amount of air supplied as above, we have:

	Barracks.	Hospitals.
Amount of air supplied per head per hour in cubic feet	998	1034

A comparison may also be made by attaching a numerical value to each order, which we may do by making the mean carbonic acid of No. 1 unity, and finding its ratio to the others thus:

No. of Order.	Mean respiratory impurity as CO_2.	Ratio, No. 1 being unity.	Differences.
1	0.1830	1.00	..
2	0.3894	2.13	1.13
3	0.6322	3.46	1.33
4 & 5	0.8533	4.66	1.20

The progression is pretty regular, and the mean difference is 1.22, which differs but little from the individual terms.

Adopting the above numbers as the respective numerical values of each order, we have for *barracks*:

No. of order.	No. of obser- vations.		Value of order.		Total.
2	89	×	2.13	=	189.57
3	88	×	3.46	=	304.48
4 & 5	97	×	4.66	=	452.02
Sums..	274				946.07

giving a mean of 3.45.

For *hospitals*, we have:

2	20	×	2.13	=	42.60
3	46	×	3.46	=	159.16
4 & 5	20	×	4.46	=	93 20
Sums..	86				294.96

giving a mean of 3.43.

Here we find the same numerical value (signifying *close*) applied to 0.580 in hospitals and 0.601 in barracks. There is thus, even in this comparatively limited number of observations, a confirmation of the opinion that more air is necessary to keep an air-space sweet in disease than in health. It is, however, right to point out that in the one case the occupation was continuous, and in the other chiefly at night only.

THE NEW METHOD OF GRAPHICAL STATICS.

APPLICATION OF THE GRAPHIC METHOD TO THE ARCH.

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One of the most important applications of Graphical Statics in point of ease of solution and saving of labor, is to the arch, both the stone and braced iron arch. We shall consider here only the first or stone arch. The method enables us to find easily and accurately both the thrust at crown and proper dimensions for stability for any form of arch and surcharge, without the aid of tables or the use of formulæ. Indeed, this is the principal advantage of the graphical method, that its solutions are general and independent of particular assumptions. We are in the present case not restricted by any special limitations, but take the arch just as it is in the case considered, and investigate it under the actual conditions to which it is subjected.

LINE OF PRESSURES IN THE ARCH.

We have already indicated (Art. 28, Fig. 16) the manner in which a number of successive forces are resisted by an arch. We see from the force polygon in that figure that the horizontal pressure is the same at every point, and that the vertical pressure is equal to the sum of the weights between the crown and any point. The pressure line is thus an equilibrium polygon formed by laying off the weights, choosing a pole and drawing lines from the pole, etc., as described in our previous articles. If the weights are small and their number great, the equilibrium polygon becomes a *curve*. This curve for equilibrium should never pass outside the limits of the arch. More than this, this curve must under all circumstances *lie within the middle third* of the arch—all its possible positions must be included between two curves parallel to extrados and intrados respectively and distant $\frac{1}{3}$ of the depth from each. The proof of this condition is simple and to be found in any treatise upon the arch. It is unnecessary to give it here, and sufficient to remark that if from any cause the curve of pressure passes beyond these limits the *neutral axis enters the cross section*. That is while on one side of the neutral axis there is compression, on the other side there is tension. But as the mortar is neglected, the joints open freely under the influence of tensile strain, all the material upon the tensile side of the axis is not brought into play at all, and might be removed without affecting the pressure upon the other side. To obtain the entire effective resistance of the

given cross section then, the pressure curve must lie between the above limits.

CONDITIONS FOR STABILITY.

The arch may fail either by rotation about one or several joints, or by the sliding of the joints upon each other. The first is effectually prevented if the pressure curve lies between the limits just prescribed. The second can never take place if the angle between every joint and the direction of the pressure at that joint is well within the *angle of repose*. The curve of pressure being known in any case, it is easy to so dispose the joints that this shall be the case. The whole problem then is simply to determine the pressure curve. The arch is stable if the joints can not slide, and if it is possible in any two joints to take two reactions, such that with the weight of the intervening portion of the arch and its load, the resulting pressure line shall lie so far within the arch that rotation about an edge cannot take place. If the arch is so light and the resistance of the material so slight that only *one* assumption of the reactions can be made, and only one such pressure curve drawn, this is evidently the true pressure curve for stability, and by it the reactions or pressures at every joint are determined.

If, however, the arch is so deep and the resistance of the material so great, that several pressure curves may be drawn, none of which cause rotation about and edge, which of all these curves is the true pressure curve?

We assert: *that is the true pressure curve which approaches nearest the axis, so that the pressure in the most compressed joint edge is a minimum.*

If we assume the material so soft that the pressure line approaches the axis so closely that only one curve is possible, then this is evidently the true curve. If now the material hardens without altering any of its other properties, such as its specific weight or modulus of elasticity, then the position of the pressure curve is not changed. As there is no reason for supposing the pressure line different in an arch built of hard material from that in one originally soft which has afterwards gradually hardened, it follows that the pressure line in all arches of same form and loading has the same position which it would have had if the arch had been originally of the softest material; that is the position which makes the pressure in the most compressed joint edge a minimum.

We have then in any case to ascertain whether it is possible to draw a pressure line, whose sides cut the joint areas within the inner third, for then since we know that there can be a still more favorable position, there is no danger of rotation.*

DIMENSIONS OF THE ARCH—STABILITY OF ABUTMENTS.

The object of the construction of the pressure curve in the arch is to determine also the stability of the abutments. When the live load of the arch can be neglected with respect to its own weight and when the material of the arch presses the usual strength and the pressure line lies within the inner third, then the lower point of rupture lies so low that the rear masonry completely encloses it. There is therefore nothing arbitrary when the form of the arch is given except the *depth*. Since in an arch of less depth than is allowable in practice a pressure line can still be inscribed, the graphical method is unable to determine the proper depth. This must be determined by practice, empirical formulae, and regulated by the strength of the material, etc. We must assume that not only the form of the arch, but also its proper depth as well as its surcharge are given. It is required then to determine the stability of the abutments.

We may regard the abutment simply as a continuation of the arch—so that the arch is continued as such, clear to the foundation; or we may regard it as a wall whose moment about the *joint of rupture* resists the rotation about this joint due to the thrust. Both views are identical, as the entire theory of the pressure curve rests upon the investigation of the rotation. They differ only in the method of expressing the safety of the abutments.

If the arch is continued to the foundation, and the space between it and the road line filled up with masonry, or if the thickness of the abutment increases

* *Colmann-Die graphische Statik.* Zürich, 1866.

from above as the pressure curve requires; or if the abutment consists of partitions and hollow spaces; still in every case the abutment is not to be distinguished from the arch proper—it is stable when the pressure line lies in the interior. If the prolonged is separated entirely from the adjacent masonry, there is no reason for not giving the axis of the prolongation the form of the pressure curve itself. If, on the other hand, there is no separation of the arch and abutment, it is sufficient that the pressure line lie within the inner third, and the abutment is certainly stable.

The supposition that the resistance of the mortar is sufficient to unite the whole abutment as a single block which turns about its under edge, gives dimensions too small. To *insure* safety it is assumed that equilibrium exists with reference to rotation about the lower edge when the thrust of the arch is 1.5 *greater* than the actual. Investigations of French engineers have showed that this coefficient of safety for very light arches is not less than 1.4. The table of *Petit* give 1.9. We assume it therefore at 2.

If therefore the *double* thrust of the arch at the lower point of rupture is united with the weight of the abutment, the resultant should *still* fall within the base. Since it is indifferent in what order the elements of the abutment are resolved, it is best to divide it into vertical slices, and unite the weight of these with the double thrust. The equilibrium polygon thus obtained should cut the foundation base within the edge of the abutment.

When the thickness of the abutment is thus determined, we must construct the actual pressure line by more than the angle of repose. Finally the pressure line itself must lie within the inner third. A single example will illustrate and apply all the above remarks, and will enable the reader to determine readily the dimensions, thrust, joint of rupture, etc., in any case.

CONSTRUCTION OF THE PRESSURE LINE—EXAMPLE.

Thus, in the accompanying Fig. 1, we are supposed to have drawn a given arch to scale. We must first divide the arch into vertical slices and determine the weight of each. If the surcharge has vacant spaces, or is generally of different specific weight from the material of the arch itself, it must first be *reduced*. Thus if the surcharge (spandrel filling, etc.) weighs, for instance, only $\frac{2}{3}$ds as much as an aqual area of masonry in the arch, we must diminish the vertical height by $\frac{1}{3}$. We thus obtain the dotted line given in the figure which forms the *limit* of the reduced laminae, and we can treat the areas bounded by this line, by the vertical lines of division and by the intrados, as homogeneous. We have next to determine the centres of gravity of the various laminae, according to the construction for finding the centre of gravity of a trapezoid (art. 33) and suppose at these points the weights, which are of course proportional to the reduced areas of the several trapezoids, to act. [*All trapezoids must be reduced to equivalent rectangles of common base, the heights of these rectangles are then proportional to their weights, art.* 33.]

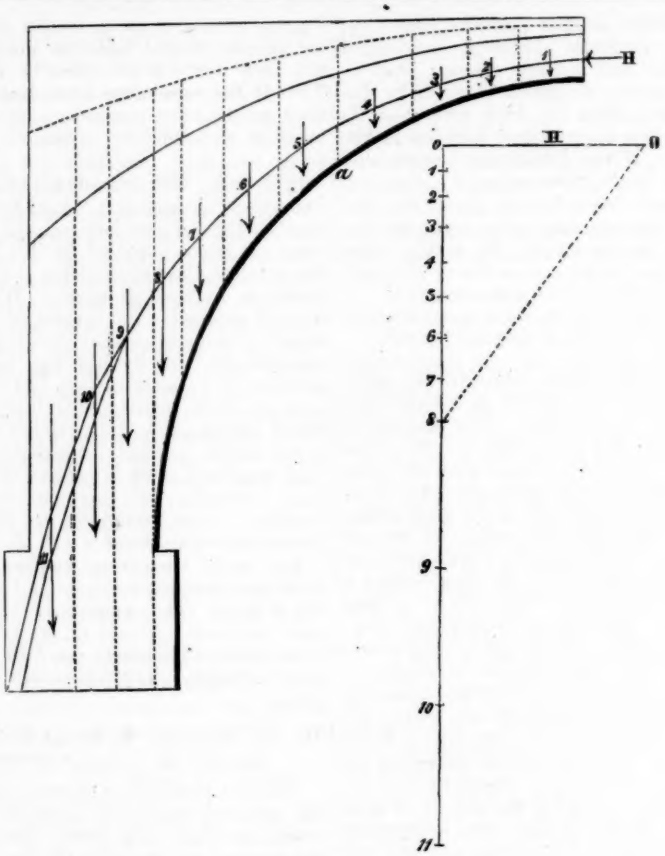
Laying off then these weights in their order we have the force line 0 1 2 3 ..... 11, Fig. 2. The weights of the abutment laminae 9 10 and 11, are laid off to same scale as the others, *one half* of their proper length. The reason will soon appear.

1st. *To determine the thrust* H *at crown, and also the joint of rupture.*

We first sketch in a pressure curve by the *eye*, and assume the point of the intrados to which this curve most nearly approaches as the edge of the joint of rupture. Draw now from the corresponding force of the *force line*, a line parallel to the direction of the assumed pressure curve at this point. This line will cut off from the horizontal through the beginning of the force line, our first approximate value for H.

Thus, suppose we have inscribed by eye the pressure curve 1 2 3 4, etc., which gives us the point *a* for the position of the joint of rupture. This point *a* belongs to force 5. Then a line drawn from 5 on the force line parallel to the side 45 of the drawn pressure curve cuts off *o* O in Fig. 2, our first value for H.

Now, assume this value of H as correct, erase the pressure curve by which we have just obtained it, and with this value of H and the forces 1 2 3 4, etc., construct the corresponding equilibrium



polygon. If *this* polygon lies always within the middle third of the arch, it may be taken as the proper pressure line and H as the true thrust. In general, however, this will not be the case. The polygon thus obtained may even pass entirely outside the arch.

We then determine another point of rupture, viz. the point of exit, or the point of the intrados to which the polygon most nearly approaches, and produce the side of the polygon at this point *back* to intersection with H prolonged through the crown. From this point of intersection draw a line which *does* lie within the middle third at the point of rupture, and *then* parallel to this line draw a line in Fig 2 from the end of the proper force, and we thus obtain a second and more accurate value of H. Erase now the preceding polygon, and with the new value of H and the given forces proceed as before, and we shall have in general a pressure line lying everywhere within the middle third. If not, another approximation may easily be made. We thus find by successive approximation, the position of the joint of rupture and the thrust at crown.

2d. *Width of Abutment.*

Since we have laid off the arch weights to scale in their true value, the pressure line thus obtained is the pressure curve for the arch. But we have laid off the abutment laminae 9 10 and 11, *one half* their true value, and the pressure line

thus obtained with the same thrust and and pole O, is the same as if we had taken their true value and *twice* the value of H. Its intersection with the foundation gives us then the proper width of the abutment for stability, according to our assumption of 2 for the coefficient of stability.

3d. *The pressure line being thus known we can easily dispose the joints so as to avoid sliding.*

Thus, by an easy construction, we can determine for any given case of arch and surcharge, the horizontal thrust, the proper width of abutments, and the disposition of the joints. If the dimensions of the arch, as given, are not such as to be stable for the load, it will be found impossible to inscribe as above a pressure line which shall lie within the middle third, and the curve of the extrados or intrados, or both will have to be altered so that this shall be possible.

The pressure line thus obtained does not indeed *exactly* correspond with the true one, as it is still possible to inscribe another which shall deviate less from it. We have also taken the double thrust for the abutment laminae alone, instead of from the joint of rupture. Both deviations render the construction more easy and rapid. It would be found very tedious to take first the *force* polygon (Fig. 2) up to about the estimated joint of rupture, then by long trial find the innermost pressure line, and, finally, after the joint of rupture is by this last line determined, to lay off the remainder of the force polygon and prolong the pressure line through the abutment.

It is far simpler to proceed as above, by assuming the point of application of the horizontal thrust, as also temporarily the joint of rupture. We obtain thus a somewhat smaller value for the width of abutment, but, on the other hand, we have for this reason taken the coefficient of stability at 2, instead of 1.9, as assumed in *Petit's* Tables.

Moreover, the widths of abutment thus obtained are greater than those obtained by the tables, as it is assumed in them that the point of application of the horizontal thrust is at the upper edge of the abutment. Thus, in every respect, the construction gives results reliable and even more accurate than the tables, as we take the arch as it really is, while in the tables suppositions are made with reference to surcharge, etc., which do not hold good for every case.

PROPER THICKNESS OF ARCH AT CROWN.

For this, as has been remarked, we must refer to practical experimental formulæ and the circumstances of the case. The proper depth depends not only upon the rise and span, but also upon the load. The pressure at the extrados at the key, which is in general the most exposed part of joint, should not, according to the best authorities, exceed $\frac{1}{10}$th of the ultimate resisting power of the material. If P is the pressure per unit of surface, H the thrust, and d the depth of keystone joint, then

$$P=\frac{2H}{d}$$

since on the assumption that the curve of pressure does not pass beyond the middle third, the maximum pressure is twice the mean pressure $\frac{H}{d}$. This mean pressure then should not exceed $\frac{1}{10}$th the ultimate resisting power of the material. "In the best works of *Rennie* and *Stevenson*, the thickness of key varies from $\frac{1}{20}$ to $\frac{1}{24}$ the span, and from $\frac{1}{12}$ to $\frac{1}{15}$, the radius of the intrados. The augmentation of thickness at the springing line is made by the Stevenson's from 20 to 30 per cent., by the Rennie's at about 100 per cent. *Perronet* gives for the depth at crown the empirical formula $d=0.0694\ r+0.325$ metres, in which r is the greatest length in metres of the radius of curvature of the intrados. For arches with radius exceeding 15 metres this gives too great a thicknes. According to Rankine $d=0.346\sqrt{r}$ for circular arches, and $d=0.412\sqrt{r}$, where r is the radius of curvature of the intrados at the curve."

"The London Bridge is in its plan and workmanship, perhaps, the most perfect work of its kind. The intrados is an ellipse, the span 152 feet, the rise $\frac{1}{4}$ as much, the depth of key $\frac{1}{30}$th the span. The crown settled only two inches upon removal of the centres."—[*Woodbury—Theory of the arch.*]

In general, we must first assume the depth at key in view of the strength of the material, the character of the workmanship, the load, etc. Then the thrust being found as above, we find the mean pressure per unit of area. If this exceeds $\frac{1}{10}$th the ultimate resisting power of the material, we must make a new supposition, increase the thickness, find the thrust and pressure anew, and so on till the results are satisfactory. The ultimate resisting power of Granite may be taken at 6,000 lbs.; Brick, 1,200; Sandstone, 4,000; Limestone, 7,000 lbs. per square foot. These values are, of course, very general, and subject to considerable variations, according to the kind and quality of the material. The strength of the material to be used must for any particular case be determined by actual experiment.

The weight of a cubic foot of stone may be assumed for preliminary investigations at 160 lbs.—brick masonry at 125 lbs.

INCREASE OF THICKNESS DUE TO CHANGE OF FORM.

Having obtained a thickness which satisfies all the conditions, we must, if the arch be very light, make some further provision for the change of form, which is sure to take place after the removal of the centre. By this change of form the pressure line is altered and the thickness must be increased. In general we need only to increase the depth from the key to the springing. This increase need not exceed 50 per cent. at the joint of rupture and weakest intermediate joint.—[*Woodbury—Theory of the Arch.*]

Thus we have all the data necessary for the investigation of any given case, and can determine by a simple and rapid construction the thrust, joint of rupture and proper thickness of the abutments, without the aid of tables or the intricate formulæ usually employed. There is no difficulty in laying down upon paper and verifying all the elements of the most complex case. The method is entirely independent of all particular assumptions, and is therefore especially valuable when irregularities of outline or construction place the arch almost beyond the reach of calculation. It is general, and may be applied with equal ease to loaded and unloaded, full circle, segmental or elliptical arches with any form of surcharge.

MECHANICAL CHANGES IN BESSEMER STEEL.

By ARCHIBALD MACMARTIN, M. E.

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The Konigin-Marien-Hutte is the only works in Germany where the Bessemer process is carried on by the direct method. The Bessemer plant there, is arranged after the true English type, and the only resemblance to the Swedish mode of procedure is the dispensing with the use of spiegeleisen at the end of the "blow"

In a new department of the establishment, started within three years, each of the converters is turned by means of a very neat and compact reversible engine, the steel shaft of which is an endless screw, which turns against the oblique cogs of a large wheel attached to the shaft of the vessel. An advantage which this arrangement possesses over the ordinary English hydraulic arrangement, is the fact that the endless screw suffices to turn the vessel, in *either direction any number of complete revolutions;* while even the latest American improvements, so clearly explained to us by Mr. Holley at the opening session of this meeting, do not secure even one complete revolution without changing the angle of inclination of the hydraulic piston.

In the endless screw arrangement, there being no limit to the working of the motor in either direction (no return-stroke necessary), the vessel can always, unless outside reasons demand the contrary, be turned around to any desired

position by the shortest cut, whether backwards or forwards. Also, the diameter of the cog-wheel attached to the vessel can be made sufficiently great to avoid all unevenness of motion. In the old department the motive-power still continues to be hydraulic.

If reports be true, the new department produces spiegelized steel, for the manufacture of all-steel rails. But the old department is still, as from the beginning, devoted to the production by the direct method of steel for steel-headed rails. The most remarkable fact connected with this direct steel is the ease with which it welds to the iron of the rail-packets, although no borax or other fluxing agent is used to facilitate the welding. It is very rarely that an exception occurs, and an ingot or a charge is discarded by the rail-mill.

The mixture of pig-iron used for the production of this steel is melted in cupolas of very interesting construction (not to be described here), and consists generally of gray "Konigin-Marien-Hutte," two grades of gray "Georg-Marien-Hutte," of Osnabruck, "Charlotten-Hutte," and "Schmalkaldner-eisen." The last is rich in manganese and resembles spiegeleisen, although its silver-white crystals are, as a rule, much smaller than those of spiegeleisen. It is probably due to a high percentage of manganese in this mixture of iron employed, that it is possible in Zwickau to do what has been tried in vain in England, namely, to dispense with the use of spiegeleisen at the end of the blow.

The five tons of molten iron are blown, till the conductor of the operation is warned by the spectroscope that the charge has come to the condition of steel. Then the vessel is turned over, back downwards, and the blast cut off. In more than ninety cases out of a hundred, nothing further would be necessary. But, to make assurance doubly sure, a mechanical test is applied. No extra time is lost by this; for it is always well to let the finished charge rest in the vessel a short period previous to pouring it into the ladle. This second test is called the "globule-test." Three or four long iron rods are plunged into the metal-bath, at the mouth of the converter, and drawn out very rapidly. The slag adhering contains minute globules of metal, of the same degree of decarburization represented by the whole bath. These, after the rods have been plunged into cold water and the slag thus disentegrated, are collected together and hammered. Those globules which cooled on the outer surface of the slag, are apt to be, in part, superficially oxydized, and are always discarded, because they are almost sure to crack on the edges when hammered. But any wholly bright globule, even slightly irregular in shape, is suitable for the test in question. A number of the chosen globules are hammered upon an anvil with a hand-hammer. If the steel be *too soft* (which almost never occurs), the globule will hammer down very flat and with unbroken edges; but the experienced hand can readily feel that the resistance offered to the hammer is too slight. If the steel be too hard, the globules will crack on the edges when hammered; or their too great resistance to the hammer can just as easily be felt, as can the opposite in the former case.

When the steel possesses the desired degree of hardness, no cracks are seen on the edges of the hammered globules; but yet a perceptible (though not too great) resistance is offered to the hammer But, if any globule that is partially coated with oxyde, or any wholly bright globule larger than 3^{mm} in diameter, hammers out without cracking on the edges, it is a sign that the steel is too soft. There is a limit, then, to the size of the globules taken.

When the steel is shown by the test to have the right hardness, it is allowed to remain as much longer in the converter as may be necessary to cool it, or to get rid of contained gases, etc.; after which it is poured into the ladle and cast into ingots, as in the English method.

When the hammered globules show too great hardness, the blast engine is started again, and the vessel again brought to the upright position, for extra blowing. But, so nearly accurate is the original indication of the spectroscope, that it is rarely necessary, in cases of insufficient previous blowing, to do more than merely turn the vessel up and then immediately down again, in order to make up the deficiency; as will be shown by making a new globule-test.

But, when the very rare case occurs,

that the metal has been blown too far, all that can be done (unless, indeed, it is possible and convenient to finish up in true English style) is to add a small quantity of manganiferous white iron (generally "Schmalkaldner") cold, and then blow a little more, till the spectroscope warns again to stop.

The use of the spectroscope in the Zwickau process is one of the most beautiful expedients in metallurgy, One never tires watching the brilliant changes in the spectrum, blow after blow. The specific causes of these changes have been the subject of much dispute and unsatisfactory investigation. But all are agreed that carbon has something to do with them, whether as such or in gaseous form in such nitrogenous compounds as cyanogen. Whatever be their cause, these changes takes place, and that so regularly that an experienced eye can place full dependence upon them as indicators of the state of preparation of the metal-bath. The spectrum at first appears without lines; but, as soon as the "spark-period" begins to give place to its successor, and the clear flame to extend out of the mouth of the converter, the bright orange-yellow sodium-line quickly makes its appearance, and remains clearly visible till the blast is turned off. After the sodium-line appear the red lines, which represent calcium and lithium; and then a beautiful series of perfectly graded green lines in the green, and pale-blue lines in the blue section of the spectrum, manifest themselves, one after another, each in its series, until, at the climax of the operation, when the greatest heat is attained, the spectrum rivals that of chloride of copper in beauty and brilliancy. A very experienced eye can also sometimes see a beautiful violet line in the violet section at this point.

But the characteristic lines of the Bessemer spectrum are the beautiful band-like, graduated series, in the blue and especially in the green section. In the inverse order to that in which they arose to their climax, these lines gradually diminish in brilliancy, and at last vanish. But some of the green lines still remain after the blue series has entirely vanished; and at this point nothing must be allowed to distract the conductor of the operation from closely watching the spectrum; for the only index (though a perfect one) of the exact end of the operation, is the degree of brilliancy or certain green lines, which remain when the charge has arrived at the point of desired decarburization. For different mixtures of pig-iron, a slight difference in the appearance of the indicating green lines is noticable at this point; and to secure, with the same mixture, a desired slight difference in the grade of steel produced in two different blows, proper allowance must be made, on one or other side of a certain degree of brilliancy of the green lines. This is merely a matter of experience, and any liability to risks, in producing either the same grade of steel with different mixtures of iron, or different grades with the same mixture, is always counteracted by the subsequent globule-test, if only the conductor of the operation be sure, when making either of the above changes, to blow his charge rather too little than too much.

With one pair of five-ton vessels and three cupolas, the ordinary production in Zwickau is twelve to fourteen blows in twenty-four hours.

The ingots, as soon as they shrink enough to be removed from the moulds, are evenly heated in a gas or air-furnace, preparatory to being hammered by a 17½-ton steam hammer, which removes their bevel, and reduces them to a uniform cross-section, a little less than the size of their original smaller end. There is no doubt that, if hammering previous to rolling is advantageous, the tremendous blows of that massive hammer are of great advantage to these ingots. Each bloom is weighed and wheeled to the rail-mill, where, after reheating, it is rolled out into what is called a "platina." One platina corresponds to the steel heads of several rails, and must be cut up into a corresponding number of pieces, of proper length for a rail-packet. The platina is a plate about eight and a half inches wide, and one inch and a half thick, with a longitudinal central-flange on its upper surface of a little more than one square inch cross-section. Each piece of platina constitutes the bottom of a rail-packet (the flange lying uppermost), and granular iron, flat rolled pieces of old steel-headed rails, etc., and piled upon, it,

on each side of its flange; and lastly a fibrous iron platina without a flange, makes a top for the packet and secures a tough bottom for the rail. The packets are brought to a bright welding heat, in ordinary reheating-furnaces, and then rolled, in two heats, into rails, there being twelve passes in the final heat. The welding is perfect, and the fracture of a finished rail shows a head completely of steel resting on two shoulders of granular iron, while a tongue of steel, corresponding to the platina flange, extends from the head one-third of the way down the upright of the rail, penetrating it like a wedge. But the bottom of the rail shows a beautiful fibrous fracture.

The use of crop-ends of steel-headed rails and pieces of broken-up old rails of the same kind, as components of the rail-packets, is worthy of notice. These pieces are first rolled out as flat as the case requires, and two lengths are usually employed in each packet. But, as the steel will not weld to itself, care is taken to lay these pieces so that the head of one piece lies against the fibrous iron bottom of the other, while a layer of granular iron always separates the platina from all parts of these old rail-sections. The crop-ends of the platinas, and those rail crop-ends not long enough for convenient use in the rail-packets, are generally rolled into rail-straps ("laschen"), or, if they are very small, they are used cold, as occasion requires, to cool down the metal, in too hot blows, previous to casting.

This utilization of old rails and crop-ends enables the managers to dispense with the use of a Siemens-furnace for working up their steel-scrap; although this was contemplated, and an agreement made with Mr. Siemens, by which Mr. Jones, of Wales, was sent to Zwickau, to assist in the arrangement and take charge of the starting of a gas-furnace for the manufacture of Siemens-Martin steel. The plan was, for the time, given up, and Mr. Jones (who has since, with me, constructed and is now running a Siemens-furnace, with the latest improvements, near Providence, R. I.) was, while the matter was in abeyance, given charge of the furnaces where the steel ingots are heated for the hammer. I was at that time (1871), through the kindness of Herr von Lilienstern, the general superintendent, allowed the free run of the works as a "volunteer;" and thus Mr. Jones and I were enabled to try experiments with the steel, aided by such useful auxiliaries as some very hot-air furnaces and a 17½-ton steam-hammer.

The experiments to be here recorded had to do with an investigation into the effects of heat upon hammered steel.

We found that the thoroughness of the hammering had nothing to do with the coarseness or fineness of the grain of steel, provided the hammered piece were subsequently exposed, for any protracted period, to a very high heat. I was, at the time, preparing a Zwickau collection for the metallurgical cabinet of the New York School of Mines, and it occurred to me to illustrate this property of steel by a series of samples. We took a small test-ingot, and, after heating it as high as the ingots are usually heated for hammering, hammered it out from its original size of 3 inches square into a bar about 1¼ inches square. The grain was then very fine throughout, just as in the ordinary hammered samples taken from every blow. The bar was then put into the furnace again and left from two to three hours exposed to a heat not quite as high as that at which the steel-headed rail-packets are rolled. It was then taken out of the furnace, and, as its outside now shows, was hammered for only one-half of its length and then bent up into a horseshoe-shape, so that its two ends could be viewed side by side. There were only four blows of the hammer given to it—one on each side; and yet, when enough of each end was broken off to show the interior structure of the two halves, a most astounding contrast presented itself. The end not hammered since reheating had a much coarser and much more distinctly crystalline structure than even the coarsest of large unhammered Bessemer ingots, while the rehammered end was just as fine in grain as the whole hammered bar had been before reheating. The fracture of the unrehammered part resembled, indeed, more than anything else, that of galena of the same degree of coarseness. This specimen, with its two contiguous fractured-ends, can be seen at any time in the metallurgical cabinet of the

School of Mines, together with samples of hammered and unhammered ingots (with which to compare it, as to grain), a section of platina, and one piece of a steel-headed rail, from Zwickau, beautifully showing by fracture the interior structure, with the wedge-like penetration of the steel-head into the iron body of the rail, and the exceedingly fibrous quality of the rail-bottom. The practical bearing of the facts proved by these samples is of more importance than may at first appear.

If we apply them to the rail manufacture at Zwickau, the question immediately arises: "Of what real benefit is the use of a steam-hammer there for blooming the steel ingots?" As everybody knows, they could be brought down to shape at much less expense by a pair of rolls, as in many other works in this country and abroad. But assuming that, of a hammered and a rolled bloom, drawn down to the same size and shape from two similar steel ingots, the former has a much more compact structure than the other, it does not by any means surely follow that the same or an analogous difference will exist, after the two blooms have been similarly heated, till they are soft enough to roll out into platinas.

But, even if experiment should prove such a difference, can it be supposed that its effects would be in any measure apparent in the final steel-headed rails made from these two different platinas? It seems to me that each of the two platinas would, just before the rolling of the rail-packet, have the same coarse structure that we see in the unrehammered section of our horseshoe-shaped sample, however great the difference in grain may have been previous to their exposure to the welding-heat. This can fairly be assumed from the fact that the specimen referred to was not exposed to a greater than a welding heat.

The application of this subject to the manufacture of all-steel rails can be satisfactorily determined only by still further experiment; because the temperature at which these are rolled is less then a welding-heat, and also the thickness of the blooms, when they last leave the reheating-furnace, is much greater than that of the platinas at Zwickau, and this would probably partly counteract the crystallizing effect of the heat. Such further experimentation would do much to throw light upon the discussion so ably carried on before the Institute, about a year ago, by Messrs. Holley and Pearse, upon this same subject of "Hammer or No Hammer?" These gentlemen have it in their power to seek, in a comparatively untried field, for a ratifying test of the correctness of their theories on this subject, and it is sincerely to be hoped that such a course will be pursued.

THE CONSUMPTION OF IRON PER CAPITA.

From the Bulletin Iron and Steel Association.

By the phrase, "consumption of iron," is meant the utilization of iron in its raw or unworked state, as pig iron, blooms made direct from the ore, castings direct from the blast furnace, and scrap iron. We include scrap iron (by which phrase we mean all old iron) because whenever used it displaces at least its own weight of pig iron or blooms. If it were not used, these would be. Correctly speaking, iron is never *consumed*. Its quantity may be slightly diminished by wear and tear and by the action of the elements, but it is never wholly lost. It can not be eaten like bread, nor burned like wood. In consuming or utilizing iron, therefore, after its conversion from the ore, we merely change its form. In an inquiry into the consumption of iron by a nation, the object should be to ascertain how much *pig iron or its equivalent* is required to meet the industrial wants of that nation. If we aim to ascertain the annual consumption of iron by that nation, evidently the quantity of iron actually consumed in any year can not be decreased upon the pretext that a portion of it had been used ten or twenty years before and cast aside after it ceased to be of service. The accept-

ance of this proposition would not lead to correct results.

In the able and exhaustive report on The Production of Iron and Steel, by the Hon. Abram S. Hewitt, United States Commissioner to the Paris Universal Exposition of 1867, there occurs the following estimate of the consumption of iron per capita in all countries at that time :

"Allowing for the production in barbarous countries, and something for the use of scrap iron, it may be stated in round numbers that the production, and consequently the consumption of the world, has reached 9,500,000 tons of 2,240 pounds each, or 21,280 millions of pounds ; so that if the population of the world has reached 1,000 millions the consumption is a little over 20 pounds of iron per head. A careful calculation, after allowing for the iron exported, shows that the consumption per head in England is 189 pounds of iron. The consumption in Belgium has reached about the same limits. The consumption in France is 69½ pounds per head, and in the United States not far from 100 pounds per head. If the industry of the whole world were as thoroughly developed as in Great Britain, the consumption of iron would reach nearly 90,000,000 tons per annum. If brought to the standard of the United States, a little less than 50,000,000 tons per annum would answer ; or if to that of France, a little over 30,000,000 tons would be required ; figures to be increased further by the steady increase of population in the world."

Since this estimate was made, statistics show that the world's annual production of iron has increased from 9,500,000 gross tons in 1867 (Mr. Hewitt's figures) to 15,000,000 tons in 1874. The increase of the population of the globe has certainly not kept pace with this increase in production ; consequently the consumption per capita has increased. It was probably over 30 pounds in 1874, against 21 pounds in 1867, as estimated by Mr. Hewitt. Making no allowance for the use of scrap iron, an estimated population of 1,100 millions in 1874 will give in the total product of cast or pig iron in that year exactly 30 pounds consumption per capita. This increased consumption is easily explained by the increased demand during the past few years for iron for railways, iron ships, iron bridges, iron buildings, iron pipe, and other comparatively new uses of iron. This stimulus to the consumption of iron has, however, been sensibly weakened in most countries since the autumn of 1873, when the American panic occurred, and it is not at all an open question whether the world's consumption of iron will increase in the same proportion during the decade which began with 1874 as during the decade which then ended. It will not. The depressing effects of the financial revulsion which has affected many countries besides our own will restrict this consumption for some time to come, particularly in the interruption to the building of railways. The extensive substitution of steel rails for less durable iron rails, and the strong tendency to substitute steel for iron in many other forms, will necessarily lessen the demand for pig iron. The increased attention now given to the reworking of scrap iron, especially in this country, while not in a strict sense affecting the consumption of iron, will also reduce the demand for pig iron. Finally, the occurrence of great wars is one of the most powerful influences in stimulating the use of iron, and it is scarcely possible that Europe and America can be convulsed during the decade upon which we have just entered by such violent and destructive struggles as the past few years have witnessed.

The consumption of iron per capita in the United States is placed by Mr. Hewitt at 100 pounds in 1867. Without inquiring into the basis of Mr. Hewitt's calculation, we proceed to inquire whether the per capita consumption of iron by this country has since advanced beyond his estimate, and if so, how much. We will first take the census year 1870, for which more detailed and reliable data exist than for any subsequent year. From the census report and the statistics of the Treasury Department we have compiled the following table, showing the quantity of pig iron or its equivalent which was actually used in the census year :

	Net tons.
Production of pig iron in the census year 1869–70.....	2,052,821
Consumption of domestic and imported scrap iron, in the census	

year, in the manufacture of 1,350,663 tons of rolled iron, 1,115,000 tons of castings, 103,388 tons of forgings, 110,808 tons of blooms, and 30,354 tons of steel..	630,442
Importation of pig iron in the fiscal year 1869–70, corresponding very closely to the census year.....	171,677
Importation of 419,924 net tons of rails, bar iron, castings, and forgings, in the fiscal year 1869–70, in approximate tons of pig iron..	493,685
Total quantity of iron made in the United States and imported in the census year 1869–70.....	3,348,625
Deduct 1,557 tons of pig iron exported from the United States in the fiscal year 1869–70, and 5,500 tons of pig iron worked into finished iron, exported in same year	7,057
Quantity of iron actually used in the United States in the census year 1869–70, the quantity held in stock at the close of the year being estimated as equal to that carried over from the preceding year....	3,341,568

The 3,341,568 net tons of iron contained 6,683,136,000 pounds, which, if divided by 38,925,598, the total population of the United States in the census year, give 171 pounds as the per capita consumption in that year. This result is so much more gratifying to our national pride than that reached by Mr. Hewitt only four years before the taking of our last census, that we were ourselves astonished by it, and we therefore give in entire frankness in the above table the process by which it was reached.

The year 1872 was probably the year of greatest activity in the consumption of iron in this country. It was the year of the iron famine, when production and consumption were both stimulated to the utmost. In the following statement we have endeavored to ascertain the quantity of raw and scrap iron used in that year. The elements of the calculation are the same as those which were employed in ascertaining the consumption in the census year, but some of the data are necessarily estimated. In the certain data we have the production for the year of pig iron, blooms, and steel, and the imports and exports of iron of all kinds; while in the estimated data we have the stocks of pig iron on hand at the beginning and end of the year, the quantity of cast iron produced by the foundries, the quantiy of scrap iron used, and the production of rolled and forged iron except rails. The quantity of cast iron and other estimated iron products is obtained by assuming that the output of the foundries, bar mills, etc., had increased from 1870 to 1872 in the same proportion as that of the rail mills, which is definitely known. To ascertain the quantity of scrap iron consumed in obtaining all these products, including rails, we have assumed that in 1872 the proportion of scrap to each of these products was the same as in 1870. According to the census returns, one-third of all the iron forged and rolled in the census year, one-eighth of the pig and scrap blooms, one-eighth of the castings, and one-fourth of the cast steel were made of scrap iron. With these explanations we submit the statement of aggregate consumption in 1872:

	Net tons.
Production of pig iron in 1872. ...	2,854,558
Consumption of domestic and imported scrap iron in the manufacture of 1,941,922 tons of rolled and forged iron, 28,000 tons of pig and scrap blooms, 1,800,000 tons of castings, and 35,000 ton of cast steel in 1872...	884,581
Production of blooms from ore in 1872.....	30,000
Importation of pig iron in 1872....	295,967
Importation of 643,639 tons of rails and other rolled iron, 5,875 tons of forging, and 407 tons of castings in 1872, in approximate tons of pig iron...	764,197
Total pig and scrap iron made and imported in 1872.....	4,829,303
Deduct 1,477 tons of pig iron, and 5,203 tons of pig iron worked into finished iron, exported in 1872 6,680	
Deduct the estimated excess of production of pig iron over consumption in 1872 300,000	306,680
Total consumption of pig and scrap iron and blooms by the United States in 1872.....	4,522,623

The above 4,522,623 net tons of iron contained 9,045,246,000 pounds. The population of the United States in 1872 we estimate at 40,500,000. These figures give us 223 pounds as the per capita consumption of iron in the United States in 1872. The increase in our consumption of iron per capita from 1870 to 1872 was the difference between 171 and 223 pounds, namely, 52 pounds, or over 30 per cent. This increase in two years is

marvelous, but it must be remembered that 1871 and 1872 were themselves marvelous years. If our premises in the two calculations we have made be accepted, no other results than those reached are possible.

The consumption of iron per capita in the United Kingdom of Great Britain and Ireland is stated by Mr. Hewitt to have been 189 pounds in 1867. It has since increased. Without making any allowance for the large consumption of scrap iron in that country, which has never been definitely ascertained, and which it is impossible accurately to estimate, we obtain from the production of pig iron alone, as will be seen by the following itemized statement, a larger per capita consumption in 1872 than in 1867.

	Gross tons.
Production of pig iron in 1872.....	6,741,929
Deduct 1,332,726 gross tons of pig iron, 296,575 tons of castings, and 1,974,236 tons of rolled and forged iron and steel, exported to other countries, in approximate tons of pig iron.....	3,603,537
Left for home consumption	3,138,392

These 3,138,393 gross tons of pig iron give us 7,029,998,080 pounds, which, divided by 31,817,108, the population of the United Kingdom in 1871, show a product of 220 pounds as the per capita consumption of iron in 1872. We have not taken into consideration the inconsiderable imports of iron in that year, which would add very slightly to the consumption. The scrap iron consumed would largely increase it.

The figures given and the facts which we have made no attempt to reduce to figures point to a much larger per capita consumption of iron in Great Britain in 1872 than in this country. But we are not prepared to accept this conclusion. A very large portion of the iron retained in Great Britain for home consumption is converted into iron ships, machinery, hardware, cutlery, etc., for sale to other countries. These iron and steel products should properly not be confounded with like products which are permanently retained in the country. In the United States, however, so comparatively small are our exports of machinery, etc., and so nearly are they balanced by our imports of similar commodities, that it is fair to assume that all of the iron nominally retained here is actually consumed by our own people.

We shall never know the exact facts of per capita consumption of iron in any country. The foregoing calculations and deductions are submitted as the re-result of a careful inquiry into the *probable* consumption by the world, the United States and Great Britain.

WATER SUPPLY AND DRAINAGE.*

By W. A. CORFIELD, Esq., M.A., M.D.

III.

SEWERS AND SEWERAGE SYSTEMS.

The water is brought into the town to be soiled, and it must be removed; and besides this dirty water which has to be removed, there is the surface water, and the subsoil water that have to be removed also; together with a quantity of refuse of all sorts, with various impurities from manufactories, from slaughter-houses, from animal sheds, together with slops from private houses, and so on. This impure water is carried away from towns by means of pipes, known as sewers, and I want at once to explain to you in a few words, the difference that is to be kept in sight between a sewer and a drain. A sewer is a pipe for removing impure water, water that has been fouled; a drain, as Mr. Bailey Denton said in a letter to the *Times*, is meant to take the wetness out of soil; it is meant to dry the soil—it is not meant to carry away impure water.

As these sewers are to carry away impure water, it is perfectly plain they must be impervious to water, or they may, on certain occasions, let it leak

* Abstract of lectures delivered before the School of Military Engineering at Chatham.

out into the subsoil of the town underneath the houses, and also into the wells, if there are any. If they are impervious, the water of the soil, the subsoil water at any rate, won't get into them and so they will not act as drains. Now you will see directly why it is necessary to drain the subsoil underneath the streets and houses. That it is necessary I can show you in a half a minute.

It has been perfectly clearly shown by Dr. Buchanan, from statistics of the death-rate of certain towns that have been sewered, that in those towns which have had sewers so constructed that the subsoil water of the town has been lowered, the death-rate from consumption has increased in a most extraordinary manner. In the case of certain towns the death-rate from consumption has been reduced by half the total number of deaths, by 50 per cent, by the lowering of the subsoil water consequent upon sewering the town as it is called. But these sewers were so constructed that they acted as drains as well. Towns which have been sewered with impervious pipes throughout, so that no reduction of the subsoil water had been effected have shown no decrease in the death rate from consumption, and some have shown an increase. So that shows you that it is necessary to drain the subsoil.

Then from the incompatibility of having pipes which both drain the subsoil and are impervious, so as not to allow of the sewage to escape from them, it has been suggested to have two systems—to have drains and sewers. Mr. Menzies has been the great advocate of having what is called the separate system. His plan was to have deep sewers, pipe sewers which are impervious, and then rather superficial drains to carry off the flood waters. This plan would not provide for actually draining the subsoil unless some special provision were made for it. The usual plan that is practised is to build sewers large enough to contain all the drainage water and any reasonable amount of storm water that may fall upon the land which is sewered, but it is perfectly ridiculous to use them for intercepting natural watercourses, as is done in so many cases.

Some sewers in the South of London actually collect water from natural watercourses which ought to be allowed to run straight into the Thames. The argument for admitting this extra amount of water into sewers is that they will be kept cleaner, and that they will flush themselves naturally, as it were. But against this is to be placed the difficulty of dealing with the increased amount of water at the outfall. I may speak to you of that, however, bye and bye.

Now for laying main sewers you must have accurate plans of the places, with the levels of the surface along the roads and the streets, and the levels of the deepest cellars, so that the sewer of the street may always be below the level of the lowest cellers. You must also know the levels of high and low tides, if near to the sea. The general plan, according to Mr. Rawlinson, ought to be made on a scale of two feet to a mile, and the detailed plan on one of ten feet to a mile.

I am now going to refer you to an important discussion that took place before the Institution of Civil Engineers in 1862 and 1863. You will find it in Vol. 22 of the *Proceedings of the Institution of Civil Engineers.* I shall have to refer to this discussion several times. The first point to be attended to in laying out main sewers is that they shall be straight from point to point. There is no reason that they should follow exactly the middle of the streets where they are not straight, but they should be made straight from one point to the next. The curves should be gentle, not greater than 22½ degrees, for instance. The junctions should be, as in the case of the main water pipes, curved. Rankine tells us that main sewers should not be less than two feet broad, and that the velocity in them should not be less than one foot in a second, for fear of choking up, nor greater than four feet and a half in a second, because with a greater velocity than this you have too much scouring. The usual plan, then, is to make these drain sewers, as I call them, sewers which are capable to a certain extent of acting as drains also. That end is often realized by setting the bricks of the invert, as it is termed, in cement, and setting the others with mortar. The bricks of sewers ought always to be set in hydraulic mortar or in cement. These drain sewers, I should

tell you, are on the plan of the oldest sewer we know of, namely the Cloaca Maxima in Rome. That Cloaca Maxima was not constructed as a sewer: it was originally a drain. A great deal of blame has been thrown upon the Romans because the Cloaca Maxima was not made impervious; but we must remember it was originally constructed as a drain. It was formed to drain off the water about the Forum, and it did so, and does so to this day. It only came afterwards to be used as a sewer, that is to say, to have refuse matter thrown into it, and that is no doubt how we have got our system of drain sewers, and there is no doubt that the sewers in many towns in England were originally built as drains.

The first thing to mention is the trench. Mr. Rawlinson tells us, in a paper that I have already quoted to you, and which is entitled "Suggestions as to plans for main sewerage and drainage," that the most difficult earth to deal with is quicksand, and as a rule it should only be opened in short lengths. The trench may require to be close timbered; and in all cases the greatest care should be exercised in taking the timbers from the sides of the trenches so that none of the side earth may fall down upon the sewers.

With regard to the depth of the trenches, of course this must vary very much in different places. The only condition is that they require to be placed deep enough to drain all the cellars. I may mention, as an example, that at Stratford-on-Avon the sewers are constructed from 16 feet deep down to 4 or 5 feet in many other parts. At Rugby they average 11 feet in depth, but they vary from 7 to 25 feet. One of those papers that I quoted to you from the *Proceedings of the Institution of Civil Engineers* (Vol. xxii., p. 265), says, that the average depth is 12 feet, but that the depths vary much. Tunneling may be required, as practised now in the large outfall sewer being constructed at Brighton. Tunneling, however, should never be resorted to when it can be helped, because much better supervision can be exercised over the construction of a sewer when you have a trench than if you have a tunnel. This is perfectly clear, and also for the same reason, night work should not be encouraged: the men should work in the day time.

Now as to the incline. The incline, we are generally told, should not be less than 1 in 600. Sometimes, of course, that cannot be got. The incline must vary very much with the natural incline of the soil. If possible, you should have it about 1 in 600 in mains, and a greater incline in the smaller sewers, and the greatest incline in the house sewers. The incline of the pipe-sewers that come from the houses should not be less than 1 in 60. Where sewers are joined the incline should be greater. Where a small sewer enters into a large one there should be a quicker incline for some little distance.

Another point is this—that a larger sewer should never open into a smaller one; neither should a sewer open into one of the same size, but always a smaller one into a larger one. The inverts should not be level. The invert of a smaller one should be higher up than that of the larger one, so that there may be a fall. "Main sewers and drains should be adapted," as Mr. Rawlinson says, "to the town area, length of streets, number of houses, surface area of house yards and roofs, number of street gullies, and volume of water supply."

With regard to the size and shape of the main sewers. The size is, of course, very variable indeed. I told you that Professor Rankine said they should not be less than 2 feet broad. They are often made less than two feet broad. Perhaps the best thing is to give you an example. It is taken from a discussion in the volume of the *Proceedings of the Institution of Civil Engineers*, which I referred to a few moments back. Mr. Newton said that "In purely urban districts a rainfall of one inch in half an hour ought to be provided for; thus, on the 29th July, 1857, he registered at Preston three quarters of an inch of rain in 35 minutes, and on the 8th October, 1861, nearly the same depth in 30 minutes. On the latter occasion an egg-shaped brick sewer, 4 feet 9 inches high by 3 feet 2 inches wide, and 300 yards in length, with a fall of 1 in 156, carried away this water from a closely built and densely populated district containing 117 acres. In another part of the town, which was also built upon, and which

contained 85 acres, a sewer 3 feet 6 inches high by 2 feet 4 inches wide, with a fall of 1 in 75, carried off these storms without causing any damage, and without the water rising in the cellars, which were generally from 1 foot to 2 feet below the soffit of the arches. In both cases, however, the sewers were under pressure, and on the first occasion the water rose 18 inches, and in the other case 1 foot, in the man-hole shafts." (Vol. xxii., p. 295.)

"The London main sewers vary from 4 feet in diameter, to 9 feet 6 inches by 12 feet in some cases. The three northern outfall sewers are each 9 feet by 9 feet with vertical sides, the southern outfall sewer 11 feet 6 inches in diameter." It is a good plan to make what are called intercepting sewers if there are considerably different levels in the town, or if the sewage has to be pumped at the outfall. This you know is done with the sewage of London on both sides of the river. There are two intercepting sewers in the south of London, and the sewage runs by gravitation, in the high level sewer, right away to the outfall at Crossness, and by the southern sewer, it runs also by gravitation, as far as Greenwich, where it is all pumped up into the outfall sewer, and then runs away to Crossness, where it is all pumped up into the Thames.

On the north side of London, there are three, and the sewage of the two lower ones is pumped up into the highest at Abbey Mills, and thence flows on to the outfall at Barking Creek.

Now for the shape. The best shape has been decided to be the egg shaped section. There are plenty of shapes in use. The rectangular section is evidently bad. The amount of friction is very great and likewise such sewers become choked up with deposit. A flat top has been used, but it is obviously bad. It is not so strong; and even the Romans, as in the Cloaca Maxima, used an arch. The best shape is an oval section with the smaller end downwards. Another advantage of this is that there is a saving of a material. Sewers less than 2 feet in diameter are better made circular. There was, for a long time, a dispute as to the different advantages of brick and pipe sewers. You will find in the 12th Vol. of the *Proceedings of the Institution of Civil Engineers* a paper, a very important paper, by Mr. Rawlinson, in which he supported very strongly the use of pipes. You will find that there was a great deal of dispute as to their efficacy. Mr. Rawlinson laid down three propositions that he thought should be borne in mind in laying out the sewerage of a town. In the first place, the sewers cannot receive the excessive flood water even of the urban portion of the site. That is perfectly true; they have in certain places been made large enough to do that. In the second place, according to Mr. Rawlinson, they ought not to be combined with the natural watercourses which drain large areas of the suburban land previous to entering the urban portion. No doubt sewers are frequently combined with watercourses which ought to go directly into the rivers. In the third place, they should be adapted exclusively to carry the liquid and solid refuse from the houses in such a manner as to cause the least possible nuisance to the inhabitants. These conclusions were then very much disputed, as also was the conclusion that sewers should be as small as possible and impervious.—The opponents, no doubt, who disputed these statements, did so from the fact that they did not sufficiently appreciate the antagonism that exists between sewers and drains. Mr. Robert Stephenson, on that occasion, expressed his "conviction that for certain localities. if pipe-drains were sufficiently strong to resist fracture, and sufficiently large to avoid being choked up, they might be advantageously employed to form the connections of houses, courts, and other small localities, with the main sewers, which should be constructed of brick, of such dimensions as to admit of easy internal inspection and repair, and be of form (except where the flow of water was at all times considerable) that the radius of the curved bottom should be able to gather a small supply of water into a sectional area affording the same hydraulic mean depth as in a pipe-drain of a diameter merely adapted to discharge the minimum flow." So that after all this discussion the result which was come to was this: that impervious pipes—glazed earthenware pipes—were, on the whole, the best for house drains, small streets, courts and places of that sort,

but that they were not advantageously to be used over 12, 15, or at the most 18 inches in diameter. Certainly, when above 18 inches in diameter, it is cheaper to make a brick sewer of oval section than to lay pipes.

In very wet soil, Mr. Rawlinson has used iron inverts to prevent the subsoil water coming into the sewer and keeping it continually full up to a certain height. Mr. Simpson has described iron pipes to be used for sewers where there are bad foundations, as in running sand; there is a plan for preventing subsoil water from getting into sewers without using cast-iron pipes, which is described by Messrs, Reid and Goddison, of Liverpool, in the British Association Report for 1870. They have introduced a subsoil drain and pipe rest to be placed beneath the pipes. It has got a section like the letter D. The pipe sewer is laid upon it, and it acts as a drain to keep the subsoil water below the sewer.

With regard to the outfalls—the outfalls ought, if possible, to be quite free. The first thing that you have to do is to choose the best place for the outfall. For that there are no general rules whatever, and you must be guided entirely by the nature of the locality. Most sewers having been originally constructed as drains, it is perfectly plain that their only outfall is into the sea or into a river, and so most of the outfalls are built into the sea or into rivers, and the sewage is thrown away. We shall in the next lectures consider some other methods of dealing with sewage.

If possible the outlet should be free. If it cannot be free there must be some means adopted for preventing the sewage getting backed up in the sewers, especially when the rivers are high, or at high tide in the sea. If the sewage is allowed to get back in the sewers you may get the cellars flooded, and you will certainly get sewer air forced up into the town. One way of preventing this is by causing the outfall to open into a large tank out of which the sewage is continually pumped. Another plan is simply to have a flap to the mouth of the sewer—a flap which shuts and keeps it full of sewage. In that case the outfall has to be made large enough to contain an enormous quantity of sewage. Then it should certainly not be taken—that is if you drain into a river—into a river near the town, and certainly not above one. The better method is perhaps to have a large tank, if the outfall sewer must be below the surface of the water. Where you have rivers with considerable difference in the level, a plan has been adopted for discharging the sewage in summer when the river is very low by means of a subsidiary pipe. This cast-iron pipe is taken at a lower level into the river. There is a valve capable of being raised by a windlass, which valve prevents the sewage coming out by the main outfall. The ordinary sewage of the town can then run away by this cast-iron pipe, and get off into the river at a lower level. This plan has been put into operation at Windsor by Mr. Rawlinson; so that the ordinary amount of sewage need not run out by the main outfall high up when the river is low, in which case it would run down the banks causing a nuisance. When there is an enormous amount of sewage and a flood in the river, and of course the river is high, then it is allowed to come out by the main outfall. When there are steep gradients in sewers there ought to be steps made, and flaps placed at the upper parts, and at such places also there ought to be ventilators. Ventilators are best constructed to open at the level of the street, and are best made in connection with man-holes. In the first place I ought to tell you that it is absolutely necessary to ventilate sewers. It is perfectly certain that a certain amount of sewer air, as it is called, is contained in all sewers, and is given out from sewers, and is given out from sewage whether there is much stagnation or not. Where there is great stagnation the more is evolved. The strongest argument for ventilating sewers is the argument used by those who say they should not be ventilated. They say they should not be ventilated because they can be securely trapped, and the small amount of gas that does accumulate in them can be prevented from coming into the houses. Now this fact that sewers need to be trapped is the best argument to show that it is neccessary to ventilate them. I should tell you that all water traps are, essentially, bends in pipes which will hold water. Water traps

are of very little use against sewer air. I do not mean to say that the air will often actually force them, though it will do that sometimes; but what I mean is that most of the dangerous elements which are the constituents of sewer air are soluble in water and are evaporated and given out, so that it is very little use to rely on water traps, especially if they are placed under pressure. Therefore ventilation must be provided.

Now this ventilation has been carried out in very various ways. The simplest way, of course, is to allow a certain number of the openings into the sewers in the streets to be untrapped, and then the sewer air escapes into the streets.

That was the plan condemned some years ago, because it allowed the sewer air to come out straight into the streets, and to become disagreeable to persons walking. That plan, however, is certainly very much better than letting it remain in the sewers, from which it will get into the houses, as it is certain to do, through weak points.

Another plan was to have special ventilating pipes carried up to the top of high buildings. These are very well in their way, but they are certainly not sufficient. Sometimes at the top of these pipes, Archimedean screws have been placed. At Liverpool an enormous quantity of these Archimedean screws have been placed at the top of such pipes, and more are now being placed. But I must tell you with regard to these that Drs. Parkes and Burdon Sanderson, in their Report lately on the Sanitary Condition of Liverpool, made experiments, and found that these Archimedean screws altered the pressure of the air in the sewers to a very trifling extent, so that they did not seem to be of any great value.

Another plan is to connect the rain-water pipes with the sewers directly, or at least some of them, and to leave them untrapped. If you do this, it is necessary that rain water pipes should be thoroughly well constructed and well jointed, or else air will escape into the neighborhood of the houses. But the best plan of all is to have plenty of openings into the main sewer directly over it, and to have special ventilating openings connected with the man holes along each sewer at certain intervals, in the middle of the streets. You must have man holes, and where a sewer makes a bend there ought to be a man hole; and there likewise there ought to be a ventilating shaft, and also at every one of the steps which I formerly mentioned to you. Where you have a steep incline you should have a step and a fall, and there, there ought to be a flap and a ventilation shaft provided with charcoal trays.

There are two or three ways of doing this, one is to have a ventilating shaft at the side of the man hole. The air that comes up the man hole passes into the ventilating shaft and through the charcoal and out into the street, and the air is deodorized by passing through the charcoal. That is one way. The dust and dirt which will collect at the bottom of the shaft can be easily removed through the man hole. Another plan is to suspend charcoal trays at intervals in the man hole itself, and then to have an opening through which the deodorized gas goes. If you have plenty of these openings along the sewers into the streets there will not be much nuisance. Then besides ventilation, sewers generally require flushing, or at any rate cleaning out. A deposit occurs in certain parts. Now the old plan used to be to make all main sewers so that a man could go through them. That plan is not now employed, because flushing has been adopted instead of cleansing out by hand labor, that is to say to a very considerable extent. Flushing is performed either by stopping the sewage at certain places and so giving it a higher head, which is the plan often adopted, or by having some special reservoirs of water (collected for the purpose) at the higher parts of the sewers which can be allowed to rush down them; or again, by making arrangements with the water companies for the supply of a sufficient amount of water to flush them continually. And they should be flushed regularly, or deposit is sure to occur. The Paris plan of flushing the sewers is interesting. You know they have in Paris enormous subways under the streets, and the sewer runs along at the bottom of the subway. This subway has a rail on each side of it, and they flush the sewer in this way: a wagon is run along these rails, and there is a flap which descends

from the wagon into the sewage below. The force of the sewage pushes this flap on, and carries the wagon on too, and the flap of course displaces everything before it. A certain amount of space is left beside the flap, so that the sewage rushes past this, and it in fact chases everything before it that stands in its way, so far as deposit is concerned. Of course the expense of flushing sewers with water is very much less than that of cleansing them by hand labor. I could give you some instances of the amounts of the cost in each instance, but I do not know that it is necessary.

We have now followed the course of water from the place where it is collected into the town, and we have also described sewers. I began by describing to you the outfalls, because that is the natural way of proceeding, not because the water followed that course, but because, before you have small drains and sewers in a town, you want the outfall and the main sewers.

I have now before I go any further a few more points to tell you with regard to the house sewers, or house drains, as they are generally called. In the first place, I have already told you that the fall of the house drains should not be less than 1 in 60. Then, the next point is that house drains ought not to run underneath the basements of houses. They generally do so, as you know perfectly well. If they do they ought to be made of impervious pipes laid in concrete. The next point is that they ought invariably to be ventilated. If the water closet system is used perhaps the best way of ventilating them is to allow the pipe which comes from the closets—the soil pipe—provided it descends outside the house, as it always should, to be untrapped at the bottom, and to be open at the top, so that the air from the house sewer finds exit into the open air continually. If the water closet plan is not adopted there ought to be one or more special pipes for ventilating the drain carried up to the highest point. Or, again, some of the rain water pipes can be left untrapped; but this is not so good a plan. If the soil pipe be inside the house it should be trapped at the bottom and ventilated at the top, and then a special ventilating pipe must be provided for the sewer. Another plan, an excellent one, in addition to this, if the house drain be long enough, is to cut it off—to make a break in it, as it were—before entering the main sewer, and that is done by making it discharge into a ventilating shaft. There is a swing flap on the end of the house drain in the shaft, which is shut except when the water is running. The air which comes up from the street sewer into the shaft cannot pass up the house drain, but ascends through trays of charcoal, and finds its way out into the open air through openings which are left between the bricks at the top of the shaft. The whole thing is covered with a stone slab, just above the level of the ground. If you want additional security you can place a syphon between the ventilating shaft and the street sewer.

Now if a trap is placed in the cellars, or basement of a house communicating with the drain, a precaution has to be taken, if there is any chance of the sewage backing up. In that case a trap has to be placed which will prevent sewage from flooding the basement. This is done by means of a heavy flap trap.

The common traps used for yards, and even for back kitchens, and so on, are what are called bell traps. They are about the worst kind of things that could be devised, and that is why I mention them. The bell trap merely consists of a sort of inverted tumbler placed over the head of the pipe that leads into the drain. The rim of this tumbler dips into a groove, which is supposed to be filled with water. The water that passes through the perforated top which is fixed on to the tumbler can find its way round the edges of this bell, as it is called, and so into the drain. The danger is this, that the instant this top is taken off it takes the bell off as well, and then sewer air can get up into the house or yard. Now these things are continually being taken off, or left off, and therefore that kind of trap should never be used. The best to put instead of it is an earthenware syphon trap. The advantage of this is, that if the top is taken off, as it continually is, to sweep the yard or basement, it does not matter at all, bacause the top has nothing to do with the trap itself. With this syphon, if you want to ventilate the drain at that particular point, you can have a hole made at the

top of the bend (some are made with a hole), and then you can carry up a ventilating pipe from it. If you have two ventilating pipes you must carry them to different heights, one not very high, and the other to a considerable height; but practically one is sufficient. Another thing that you can do—especially if the trap is in the basement of the house—you can make any waste pipe end in the side of it, through a hole in the side above the water, and yet below the cover, so that you do not get the place flooded if the holes in the cover are stopped up, as they are apt sometimes to be. This is a very convenient plan.

Sinks ought always to be against external walls. They almost always used to be built (and now often are) against internal walls. Their pipes have no more business to go straight into drains than the waste pipes of cisterns; they should always be carried out into the yard, and made to end over one of these traps, or else they should be carried into the side of it. The same thing is true of rain water pipes. Unless rain water pipes are constructed with the view of ventilating the sewer, and are made with proper joints, they ought to end *above* the traps.

We now come to the consideration of the disposal of a particular kind of refuse matter, namely, excretal refuse matter. I want first to prove to you that it is necessary to get rid of refuse matter generally, and especially so of this particular kind of refuse matter from the neighborhood of habitations. I could quote to you from any number of reports showing that the general death rate, and also the death rate from certain specific diseases, especially typhoid fever and cholera, depends to a very great extent upon the amount of filth, and especially of excretal filth, that is in and about the habitations of people.

Take the following opinion from the evidence given by Mr. Kelsey before the Health of Towns Commission (1844). When asked, "Does the state of filth and the effluvia caused by defective sewerage, by cesspools or privies, and decomposing refuse kept in dust bins, powerfully affect the health of the population?" he says, "Yes, it does; it always occasions a state of depression that renders persons more liable to be acted upon by other poisons, even if it be not the actual cause of it. The line of habitations badly cleansed, and in this condition, *almost formed the line of cholera cases.*"

Then, after a description of cellar dwellings, which are even now prevalent in some of our large towns—in this case referring to Liverpool—Dr. Duncan pointed out that the ward "where the largest proportion (more than one half) of the population resides in courts or cellars, is also the ward in which fever is most prevalent, 1 in 27 of the inhabitants having been annually attended by dispensaries alone;" and he remarks that "people do not die simply because they inhabit places called courts or cellars, but because their dwellings are so constructed as to prevent proper ventilation, and because *they are surrounded with filth*, and because they are crowded together in such numbers as to poison the air which they breathe."

Well, then, illness is caused if these refuse matters are not removed from the neighborhood of habitations, and illness with all its attendant misfortunes and difficulties.

Now what plans have been adopted for removing these matters from habitations? That is one thing to be considered; and another thing to be considered is, are these excretal matters of any value; can anything be done with them; and if so, how can the most be got out of them?

Now, if I tell you what their composition is, you will see at once that they must be of considerable value; and when you reflect that these refuse matters constitute a great proportion of the refuse matters of our bodies, you will see at once that they must contain the same elements as our food, and that therefore there is at any rate a possibility of their being used for the reproduction of food. Now, what is their composition? The results of a great number of analyses, which are, however, only sufficiently complete in the case of males of from 15 to 50 years of age, show that the mean amounts in ounces of the various constituents during 24 hours are as follows:

	Fresh Excrements.	Dry Substance	Mineral Matter	Carbon.	Nitrogen.	Phosphates
Fæces.....	4.17	1.041	0.116	0.443	0.053	0.068
Urine.....	46.01	1.735	0.527	0.539	0.478	0.189
Total....	50.18	2.776	0.643	0.982	0.531	0.257

[Paper read by Mr. Lawes, F. R. S., before the Society of Arts, March 7th, 1855.]

Now, what does that mean? Nitrogen and phosphates are the very things we get to use as manures, and we can from this chemical composition of the excreta calculate their relative and absolute value.

In the first place I want to point out to you that the amount of valuable matter contained in the urine in the 24 hours is considerably greater than that contained in the fæces in 24 hours. Now, that I tell you at once, in order that you may not run away with the fallacy that is sometimes indulged in that you may throw away the urine of a population so long as you retain the fæces, and that you will get the greatest amount of manure from the latter. On all heads the matters contained in the urine are in larger proportion than in the fæces, and especially as regards the important matters, *e. g.*, the nitrogen is about nine times as much.

To estimate the value, it is convenient to take amounts that are passed in a year, and it has been calculated that the average amount of ammonia—representing the nitrogen in the form of ammonia—discharged annually by one individual, taking the average of both sexes and of all ages, is about 13 lbs., or nearly that; and it has been estimated that the money value of the total constituents of the excreta is, in urine, 7s. 3d., and in fæces about 1s. 3d., giving a total of about 8s. 6d. a year, so that you see at once that the value of the urine is about six times as much as the value of the fæces. When you consider that about ten times more urine is passed (by weight) than fæces, you see that fæces are more valuable than urine, weight for weight, although the total fæces are much less valuable than the total urine. There is, then, no doubt about the value.

The next thing is, what are the plans that have been attempted for utilizing it? The earliest plan, and one that is defended by many up to the present day—and by many, I was going to say, who ought to know better—the earliest plan consists in keeping the fæces, and a certain small amount of urine for a longer or shorter period in or about the premises in some form or another. And there are two ways in which this can be done. It can be done, as it is in many towns even now, as it is notably in many continental towns, as Paris and Berlin and Vienna. It can be done either by keeping these matters in a semi-liquid state, in tanks or vessels prepared to receive them, and emptying these at certain times and taking their contents away to be used as manure, or it can be done by mixing these matters with certain refuse which will to some extent dry them; and some such refuse is found in all houses, and is, to wit, ashes. Now, those are the two plans that have been adopted—I may almost say from time immemorial, at all events for a great many years—in order to collect this valuable manure; that is to say, by those who have made any attempt to collect it at all.

Let us take the first plan and consider it for a few minutes—the plan of digging a hole in the ground and throwing all this refuse matter into it. When this is done, unless the hole in the ground is impervious, a great amount of this refuse matter will percolate into the soil around, and get into wells. In certain towns this has been actually encouraged. There are certain towns where holes have been made to receive the refuse matters of the population in pervious sandstone strata, with the express, distinct, and avowed object of letting the liquid matters, and as much as possible of the solid matters, precolate the soil and get away as best they could. These dumb wells, as they are called, have been made and shut up

with the deliberate intention of not being opened for many years, and in certain places the soil has been so absorbent that when opened the wells have been almost invariably found empty. Now that plan need only be stated to be condemned. In all these towns the well water, which is often the only supply for the people, is largely polluted, and is in fact to a great extent supplied by these very dumb wells, which are often close by; and in almost every town where there is an epidemic of typhoid fever you find an inspector going down from the Local Government Board, and reporting that this is the case.

Now, the improvement on this bad plan, or want of plan, in places where it is not done away with, is to line the pits with cement, and to provide a drain from them into the nearest sewer. Thus the cistern becomes merely a pit in which to collect the solid matters, while you allow the liquid matters, which are the most valuable, and which are just as likely to become offensive, to run into the sewer. You collect the solid matter which is less valuable, and which is rendered still less valuable by having much of its valuable material dissolved out, by the liquid which is allowed to run away.

The other plan is to do as is done in Paris, to make these large cesspools (so large that they take six months, or even a year, to fill), under the houses or under the courts, to make them impervious, and not drain them at all. Of course the pits are only theoretically impervious, but practically very many of them certainly are not so. But, however, supposing that they are, they in any case requiring a ventilating shaft, or the foul air which collects in them will find its way through, somehow or other, and will poison the air of the house. Another danger is, that if they are not ventilated, and even sometimes if they are ventilated, the men who go into them may be suffocated by the poisonous gases accumulated in them.

The first of these plans, in which the liquid matters all run away, is confessedly a failure. You deliberately take and throw away all the most valuable part, and the part which remains is not only of no value, but is a distinct expense, because no one will take it away unless he is well paid for it, so that there is a very considerable loss on that system. And so the system cannot be called one of utilization. Then, the disadvantage of the Paris plan, apart from the general disadvantage of having such a thing as an immense cesspool underneath each house, is found in the emptying of them. This operation causes a fearful nuisance, even although they are now emptied by means of carts in which a partial vacuum is first created, so that when the hose is attached to the cart and placed into the pit the semi-liquid stuff rises up and fills the tonneau, as they call it. And then I may tell you, as a matter of fact—I could give you the figures—that the system does not pay; that the collection costs so much that the manure made from the stuff does not pay the cost of collecting.

Let us consider, now, some improved systems in which this manure is collected, mixed with ashes and household refuse, and sold in a semi-dry state, because this is a plan which has been very much defended of late. These plans are developments of the old midden—a heap in the yard at the back of the house, into which all kinds of refuse were thrown.

The first improvement, as in the case of the cesspool, was to make a kind of pit lined with cement. There are different contrivances employed. There is one which is known as the Manchester plan, and another known as the Hull plan, and so on; but in all the ashes are thrown into the pit, so as to make a semi-solid mass.

The conditions necessary for them are these:—In the first place they must receive no moisture from the soil around. In the second place, they ought to allow no liquid to escape from them, because, if so, they are confessedly failures. It is plain that the object of all these systems where the excretal matters are to be kept out of the sewers must be to separate them *entirely* from the sewage, because, if you do not, you have still sewage to treat. We have already seen that the water supply of the town goes into it to be soiled, and you require sewers to take it away. You have got, therefore, water which is, to a certain extent, dirty. The theory of persons who support those systems which I am now describing is that, if you prevent the most foul part of the refuse matters from getting to the sewers, you will not then

require sewers so large to begin with; and, also, that you will not require to treat the sewage afterwards, but will be able to turn it into a river without any disadvantage. Now, if these cesspools and these midden closets require to drain their liquid contents into the sewers, the sewage will certainly have to be treated just as much as if you were to allow the whole of the refuse matter of the town to get into the sewers; and that is proved by the fact that the sewage of towns where you have cesspools and midden pits drained into the sewers is considerably more foul, and is within a very little of being as strong, as the sewage of water closeted towns, so that it requires treatment at least as much as the latter does.

The next condition with these midden heaps is that they must secure, practically, as much dryness of the contents as possible; and then they require an efficient covering up of the refuse matters by the ashes that are thrown down, and that is done in various ways. Lastly, they require ventilating.

Now, with either of these systems, it is desirable to have the receptacle as small as possible; it is desirable for sanitary reasons, though not for economical ones, to have the receptacle as small as possible, so that as little of these matters shall be retained about the premises as possible. The midden closet used at Hull consists of an impervious receptacle, which is not sunk into the ground at all, but is, in fact, merely the space directly under the seat of the closet, the front board being movable, so that the scavengers can get the stuff when full. That, no doubt, is by far the best of these simple ash closets.

After this we pass on to a still greater improvement, to what you might call a temporary cesspool—that is to say, a simple tub or box placed underneath the seat to collect the excretal matters, these tubs or boxes being collected every day by contractors and their contents used for manure.

I may tell you at once that this is really the system out of which most is got in the way of profit. There is no doubt of it whatever. This is the system that has been practiced in China for thousands of years. It is the system which is practiced now in the neighborhood of Nice, where they grow orange trees and scented flowers, and where they grow a large quantity of things which require rich manure; and it is perfectly certain that it is the system in which there is least waste.

Now, this system has been very much revived of late in certain towns. In Edinburgh and Glasgow, and especially in many foreign towns — in Berlin, Leipsic, and in Paris — this is a plan which is adopted on a large scale. Of course, the difficulties of the system are enormous, and the nuisance is considerable. As far as the difficulties are concerned, I may tell you what Dr. Trench, of Liverpool, has calculated in regard to that town; he calculates that the space that would be required for the spare receptacles for the borough of Liverpool would be 11 acres, 2 roods, 32½ perches; that if put on a railway four-abreast they would extend a distance of 12 miles. Now, you see at once that a system which requires anything of that sort is not a system likely to be adopted for any large town. But, mind, there is no doubt whatever about this, that for small places it is an infinitely more healthy and more reasonable system in every way than either of the other two plans that we have considered. It is carried out with a simple bucket, in which the refuse matters cannot be allowed to remain for a long time, because it is not large enough, and because they would become too offensive. This system is evidently better for health than keeping the matters about the premises for a long time in any other form whatever. A variety of this plan is to be found in what are called the trough latrines that are used in many large manufactories. This simply consists of a trough which runs below a row of seats, which trough can be emptied into barrels by lifting a plug at the lower end; the stuff is taken away to farms.

There are two or three varieties of the tub or pail plan. One is known as the Goux system, and another as the Eureka system; these systems have been much praised of late. They are simply pail systems, in which some deodorizer or some absorbent is used. In the Goux system there is a sort of double pail, with an absorbent between the two pails; and the idea is to do away with some of the

offensiveness of the tub or pail system.

Now, I must say a few words to you about the dry earth system, which has been so much praised. In the first place I may tell you that sifted ashes are sometimes used instead of dry earth, because they are always at hand, and that there are several plans for the use of sifted ashes which are attempts to obviate the difficulties which are met with in the procuring of dry earth. It is found that when a sufficient quantity of dried and sifted earth, especially of particular kinds, is thrown upon refuse matters, that they are deodorized, and that they may be kept for a very long time without becoming offensive. The conditions are these: in the first place the earth must be dried, and in the second place it must be deposited on the refuse matters *in detail*, as it is called, that is to say, you must not take a great heap of excretal matters and throw a lot of earth upon it, but you must throw a little earth upon it each time the heap is increased by any more refuse matters. It is found, then, that about one pound and a half of dry earth is sufficient to deodorize the excretal matters that are passed at one time by an individual. With regard to the kind of earth, almost any earth will do except sand and chalk.

The next point is, that such earth may be used several times over. After it has been used once, it requires merely to be dried again and sifted, and you cannot tell it at sight at all after it has been used two or three times from that which has been used once; you do not see any difference whatever; all the organic matters and all the matters that would be offensive are entirely absorbed and rendered inoffensive to the smell, and so long as it is kept dry this earth remains quite inodorous.

There are all sorts of forms of closets, and so on, that have been contrived for utilizing dry earth in this way, but there is not the slightest necessity that I should describe these plans to you. I will therefore go on now to tell you of the results that have attended the application of this system at various places, and the advantages and disadvantages of the system. In the first place, with this system it is a *sine qua non* that no liquids are to be thrown into the earth closet, so that it is a system which does not provide for slops; that is against it to begin with. Then if any liquids are accidentally thrown in, or if, as is the case in certain places, the air is exceedingly damp, or if the contents get moist in any way, you have, to all intents and purposes, a cesspool without its advantages, or without the special precautions that are commonly taken with regard to cesspools. That is another disadvantage of the system, we shall find more directly.

Now for the advantages. The advantages, perhaps, are best shown by giving some statements as to the working of the system at different places. At Broadmoor the lunatic asylum is supplied with earth closets. The water closets with which the place was originally supplied were done away with, and the earth closet system adopted. A mixture of earth and ashes is used, but the slops are allowed still to pass through the drains. Here, you see, you have got every advantage that such a system could have. You have sewers originally made by Mr. Menzies, made for the water closet system; and so they can send just what they like into them, and treat the earth closet in a sort of drawing-room fashion—if I may so call it—I mean give it its best chance.

Then at various schools it has been found to answer very well. The simplest form of it is a mere trough into which the refuse matters fall and into which earth is thrown. At various jails the plan has also been used with considerable advantage. At one place, where there was an attempt made to save all the fæces and urine of the boys in the school in this way, it was found that four pounds of dry earth a day* was required for each boy. I mention that to you to show you the absolute impracticability of doing a thing of that sort on a large scale. The expense, of course, would be enormous.

Where the plan has been used as a temporary arrangement it has, on the whole, succeeded very well, and especially so, for instance, at Wimbledon Camp. At Wimbledon Camp there is no doubt it has been an enormous improvement on the old system. Now you see at once that that was a temporary

* That would be 125 tons a week for a population of 10,000.

arrangement, that they could get plenty of earth, and so there was very little to be wondered at that a system which does, if it is properly carried out, deodorize offensive matters should have been there so far a success.

Now, I must tell you a little about the Indian experience. The Indian experience has been unfavorable to this system, but there are many statements made in Indian reports to the effect that it is a considerable improvement on some of the systems that were in vogue before. That you would easily believe if I read you a description of some of the systems, or, rather, of the want of system, that they had before they adopted this plan. The Army Sanitary Commission make the following statement:—"It is insufficient to remove only one class or cause of impurities, and to leave the others; and no sanitary proceeding which does not deal effectually with all of them can be considered as sufficient for health."

"The following sources of impurity require to be continually removed from inhabited buildings in India as elsewhere; (*a*) solid kitchen refuse including *débris* of food; (*b*) rain water which would if left in the subsoil tend to generate malaria; (*c*) all the water brought into the station except that which accidentally evaporates. This water is used for drinking, cooking, washing, baths and lavatories. The amount cannot be taken at less than twelve gallons per head for every healthy man, woman, and child, including servants; from thirty to thirty-five gallons per head for every sick man per day, exclusive of water for horses. . . . Practically, this water in all climates, but especially in India, becomes, if not safely disposed of, an inevitable source of disease and ill-health. It contains a large amount of putrescible matter, and if urine were mixed with it, it would become so noxious that it would matter very little whether or not the contents of latrines were added to this other sewage; (*d*) the matter from latrines, including solid and fluid excreta at about one pound per man per day, or, in round numbers, half a ton per day per thousand men."

Those are the matters which will require to be removed. Now the Commissioners go on to say that the solid *débris* being removed by hand or cart labor, the refuse water must "either be passed into cesspits, or it must be carried away, or it must be allowed to find an outlet where it can by surface drains—probably into the sub-soil."

Then, farther on, they say that the latrine matter, with which alone the dry earth system proposes to deal, "is to the fluid refuse of barracks, hospitals, cook houses, and so forth, as 1 to 190; that is, for every pound of human excreta removed under the dry earth system there are in every well regulated establishment about 190 of fluid refuse which must be otherwise disposed of." You see at once that this is absolutely condemnatory of the system for use in permanent barracks, and I think you will come to the conclusion that I have come to, namely, that it is a system that is only fit for temporary places, like the camp at Wimbledon. It is perfectly plain that it is absurd to have two systems, a system of sewers to carry away the foul water of a station—for that you must have—and another system for carrying away a certain portion of the excretal matter, which might all perfectly well be allowed to go away with the foul water.

As for the utilizing of it in this way, I do not believe in it at all. I mean to say that the cost of bringing in dry earth into a station, and especially into a large town, and then of carrying it away again, would be considerably greater than the money that would be got for the manure. You will see, and I dare say you have seen, that the manure collected under the dry earth system has been put down as worth all sorts of fabulous sums. Well, it is not worth anything of the kind. It is the greatest mistake to suppose so. The manure from the dry earth system is a good garden soil, and it is not anything more. It is not a manure. And this earth that has been passed three times through the closets is nothing more than that, as you will see in the report of the British Association Sewage Committee. In that report there are given the results of analyses of the earth after passing once, and after passing twice, and after passing three times through the closet, and it is said that after passing three times through it is nothing more than a rich garden soil, and it will not pay for in-

curring the expense of carriage to a long distance; so that the utilization question is certainly not met by the dry earth system. This is what I wanted to come to.

I may as well tell you that it has been attempted to apply this system to a town. One part of Lancaster is supplied with dry earth closets, and there are several villages in which it has been tried. In villages it seems to answer very well when looked after, In a town, for some of the reasons that I have given you, it fails. There is no doubt about that.

That will finish our consideration of the systems which propose to separate the excretal refuse as if it were something totally, and entirely, and essentially distinct from all other refuse matter forming the sewage of a town. Those systems all go upon a wrong principle. This refuse matter is dangerous to health, and those systems, one and all, go upon the principle that these matters may be retained in and about houses as long as possible, so long as they do not create a nuisance, or so long as they are not felt to be a nuisance. Now that position is obviously wrong. All these systems depend upon leaving such matters as long as possible about the houses. The object of them all is to produce a certain result with as little expense as possible. It is perfectly plain that the longer this refuse matter is left about houses, under all these systems,—I do not care which one you take,—the cheaper the plan will be carried out, so that there is a tendency in all these systems to leave dangerous refuse matters about premises for a very long time. Now, the answer is that they are perfectly deodorized or disinfected, as the case may be. The answer to that is—if your system is perfect, they are deodorized in some cases; for instance, in the dry earth system they are deodorized. But if they are not, all the danger arises that could arise from any other of the bad systems I have described to you. Then, again, the fallacy is entertained that deodorization and disinfection mean the same thing. It is certain they do not. We know quite well that the dry earth system deodorizes refuse matters. They do not putrify and cause offensive smell after deodorization, but we do not at all know that that system disinfects matters; and there is not the slightest reason for supposing that this earth, if at any time rendered moist,—and we do not know whether or not disinfection takes place even when dry,—that this earth may not then be dangerous and have infecting properties. I mean to say we do not know that the excreta of cholera patients, or typhoid fever patients are disinfected, as well as deodorized, by this mixture with dry earth, and so I think you will all agree with me that the plan which has for its principle the removal of these excretal matters immediately from the vicinity of hibitations (utilizing them afterwards, if possible), that the plan which goes upon the principle of removing them in the cheapest way possible, viz., by water carriage and by gravitation, removing them at the same time with all the other refuse matters of the population (with the single exception of the ashes), I think you will agree with me that that is, after all, the most reasonable plan. And I do not hesitate to say that nothing has contributed so much to lower the death rate of towns as the introduction of the water carriage system.

THE tunnel which has been bored under Durdham Down, and which as forming one section of the works of the Clifton Extension Railway will put the Great Western, Midland, and Bristol and Exeter systems into direct communication with the Channel Docks at Avonmouth, being now completed, was passed through by the Mayor of Bristol, Mr. C. J. Thomas, and a number of citizens interested in the Docks and railway extension. The length of the tunnel is 1737 yards, and the gradient throughout 1 in 64. It has been bored through rock of the hardest description, every foot of which had to be blown away; about 104,000 cubic yards or 250,000 tons of rock had to be got out. Mr. W. J. Lawrence, who is constructing the Channel Docks, was the contractor for the work, but the boring of the tunnel was sublet to the Machine Tunneling Company, and has been carried out under the personal superintendence of their representative, Mr. Bell, C. E. It is expected that Colonel Yolland will make the official inspection of the tunnel and line.

THE INDIAN TRIGONOMETRICAL SURVEY.

From "Nature."

One does not usually expect to find much of general interest in the Report of a Trigonometrical Survey. Col. Walker's admirably drawn-up Report, however, includes some matter of more than special value; indeed, many of the details connected with the immediate work of the Survey are calculated to interest the general reader, they are concerned to such a large extent with the peculiar difficulties to be overcome by the various parties, difficulties which make ordinary survey work look like mere child's play.

The Index Chart prefixed to the Report enables one to form a very full idea of the work which has already been done, and of how much there is yet to do. From Cape Comorin to Peshawur and all along the Himalayan frontier, and from Kurrachee on the west to Burmah on the east, the country is covered with an intricate net-work of triangulation, including, however, many gaps which will take many years to fill up. Shooting out from the northern border of the system of triangulation are numerous aurora-like lines indicating the secondary triangulation to fix the peaks of the Himalayan and Sooliman ranges. We cannot go into the details of the work of the Survey, and must content ourselves with a brief summary of the out-turn of work during the year under review, and with a reference to a few of the more interesting side topics.

Of Principal Triangulation, with the great theodolites of the Survey, seventy triangles, embracing an area of 7,190 square miles, and disposed in chains which, if united, would extend over a direct distance of 302 miles, and in connection with which three astronomical azimuths of verification have been measured. Of Secondary Triangulation, with vernier theodolites of various sizes, an area of 5,212 square miles has been closely covered with points for the topographical operations, an area of 3,650 square miles has been operated in *pari passu* with the principal triangulation but exterior thereto, and in an area of 12,000 square miles—in the ranges of mountains to the north of the Assam Valley which are inhabited by independent tribes — a large number of peaks have been fixed, many of which have already been found serviceable in the geographical operations now being carried on with the military expedition against the Dufflas. Of Topographical Surveying, an area of 534 square miles has been completed in British portions of the Himalayas, on the scale of one inch to the mile, an area of 2,366 square miles in Kattywar on the two-inch scale, and areas of 690 and 63 square miles respectively, in Guzerat and in the Dehra Dun, on the scale of four inches to the mile. Of Geographical Exploration much valuable work has been done in Kashgharia and on the Pamir Steppes, in connection with Sir Douglas Forsyth's mission to the Court of the Atalik Ghazi, and several additions to the geography of portions of the Great Thibet and of Nepaul have been obtained through the agency of native explorers.

In the course of the operations of the year under review the northern section of the Brahmaputra Meridional Series has been completed whereby two important circuits of triangulation formed by it with the Assam and East Calcutta Longitudinal Series to the north and south, the Calcutta Meridional and the Eastern Frontier Series to the west and east, have been closed. The Straits of the Gulf of Manaar have been reconnoitered, with a view to connecting the triangulation of India with that of Ceylon, which has been found to be feasible.

Probably the most important features in the operations of the principal triangulation of the year are the resumption of the chain of triangles in Burmah, and the completion of the Bangalore Meridional Series for the revision of the southern section of the Great Arc.

Referring to the revision of certain important triangulations which were originally executed at the commencement of the present century with very inferior instruments, Colonel Walker expresses his conviction that no portion of the principal triangulation remains which will ever require to be revised, and that

the last of the old links in all the great chains of triangles which might with any reason have been objected to as weak and faulty, have now been made strong and put on a par with the best modern triangulation.

The pendulum observations have been completed, and the final results are now being computed and prepared for publication.

Considerable assistance was, moreover, rendered to Col. Tennant in the operations connected with the observation of the Transit of Venus; the Appendix contains Mr. Hennessey's account of his observations at Mussooree, the details of which have already appeared.

The reports of the various district superintendents are very full, and contain a good deal that is of general interest; the accompanying district sketch-maps are of great use in enabling one to read these reports with understanding. We shall briefly refer to some of the points of more general interest.

In Major Branfill's report on the Bangalore Meridional Series, a very interesting phenomenon is noticed in connection with the Cape Comorin base-line. The operations of 1873–74 were intended to close in a side of the polygon around the base-line which had been completed in 1868–69; but it was found that one of the two stations on the side of junction had disappeared. This station was situated on a remarkable group of Red Sand Hills, where, in 1808, Col. Lambton had constructed a station by driving long pickets into the drift sand; in 1869, Major Branfill, finding no trace of these pickets, had caused a masonry well to be sunk to a depth of ten feet, where it reached what was believed to be firm soil below; but during the interval of four years this well had been undermined, and nothing remained thereof but some scattered *débris*. It would appear that the sand hills travel progressively in the direction from west-north-west to east-south-east, which is that of the prevailing winds in this locality; if Col. Lambton's station was situated on the highest point of the hills and in a similar position relatively to the general mass as Major Branfill's, then the hills must have traveled a distance of about 1,060 yards to the E.S.E., for the results of the triangulation show that this is the distance between the positions of the two stations; thus the rate of progression would be about seventeen yards per annum. From Major Branfill's Notes on the Tinnevelly district, which are appended to the General Report for 1868–69, it appears that certain measurements of the eastward drift had made it as much as 440 yards in the four years 1845–48; but the distance between the trigonometrical stations of 1808 and 1869 probably affords the most accurate measure which has hitherto been obtained of the rate of progress of this remarkable sand-wave, which gradually overwhelms the villages and fields it meets with in its course, and has never yet been effectually arrested; numerous attempts have been made, by growing grass and creepers and planting trees on the sands, to prevent the onward drift, but they have hitherto been unsuccessful.

Mr. Bond, one of Major Branfill's staff, managed to procure an interview with a couple of the wild folk who live in the hill jungles of the western Ghats, to the southwest of the Palanei hills. A strange dwarfish people had often been heard of as frequenting the jungles near the station of Pemalei, in the north-west corner of the Tinnevelly district, but until Mr. Bond caught these two specimens no trace of them had been seen by the members of the Survey. These two people, a man and a woman, believed themselves to be 100 years old, but Mr. Bond supposes the man to be about twenty-five, and the woman 18 years of age. "The man," Mr Bond states, "is 4 feet 6½ inches in height, 26¼ inches round the chest, and 18½ inches horizontally round the head over the eyebrows. He has a round head, coarse black, woolly hair, and a dark brown skin. The forehead is low and slightly retreating; the lower part of the face projects like the muzzle of a monkey, and the mouth, which is small and oval, with thick lips, protrudes about an inch beyond his nose; he has short bandy legs, a comparatively long body, and arms that extend almost to his knees; the back just above the buttock is concave, making the stern appear to be much protruded. The hands and fingers are dumpy and always contracted, so that they cannot be made to stretch out quite straight and flat; the palms and fingers are covered with thick

skin (more particularly so the tips of the fingers), and the nails are small and imperfect; the feet are broad and thick skinned all over; the hairs of his moustache are of a greyish white, scanty and coarse like bristles, and he has no beard.

"The woman is 4 feet 6½ inches in height, 27 inches round the chest (above the breasts), and 19½ horizontally round the head above the brows; the color of the skin is sallow, or of a nearly yellow tint; the hair is black, long and straight, and the features well formed. There is no difference between her appearance and that of the common women of that part of the country. She is pleasant to look at, well developed, and modest." Their only dress is a loose cloth, and they eat flesh, but feed chiefly on roots and honey.

"They have no fixed dwelling places, but sleep on any convenient spot, generally between two rocks or in caves near which they happen to be benighted. They make a fire and cook what they have collected during the day, and keep the fire burning all night for warmth and to keep away wild animals. They worship certain local divinities of the forest, Rakas or Rakari, and Pe (after whom the hill is named, Pe-malei)."

The woman cooks for and waits on the man, eating only after he is satisfied.

The means taken for tidal observations in the Gulf of Kutch promise to lead to valuable results. The object of these observations is to ascertain whether secular changes are taking place in the relative level of the land and sea at the head of the gulf. Very great difficulties were found in selecting suitable stations for fixing the tide-gauges, as the foreshores of the gulf consist mainly of long mud-banks, which often stretch miles into the sea, and are left bare at low water, when they are intersected by innumerable tortuous and shallow creeks, whose shifting channels would be very unfavorable positions for tide-gauges. Only three points suitable for tidal stations were met with on the coasts of the gulf: at Hanstal Point, near the head of the gulf; at Nowanar Point, half way up, on the Northern or Kutch coast; and at Okha Point, on the southern coast, opposite the island of Beyt. None of these points, however, are situated in ports or harbors, where piers, jetties, landing-stages, or docks might have been utilized; on the contrary, they are all situated at some distance from the nearest inhabited localities, and present no facilities whatever. The operations had thus to be of the very simplest nature. The only practicable plan was to have the tide-gauges set up on shore, over wells sunk near the high-water line, and connected with the sea by piping. The wells are iron cylinders, with an internal diameter of twenty-two inches, which slightly exceeds the diameter of the float; the cylinders were made up in sections of fifty inches in length, the lowest of which is closed below with an iron plate, and the whole, when bolted together, forms a water-tight well, into which water can only enter through the piping for effecting connection with the sea. The piping is of an internal diameter of two inches, which has been computed to be sufficient to permit of the transmission of the tidal wave to the well without sensible retardation. Iron piping is laid from the well to the line of low water; it is brought vertically up from the bottom of the well nearly to the surface of the ground, and is then carried down to the sea, where flexible gutta-percha piping is attached, and carried into the deep water. The outer piping terminates in a "rose," which is suspended a few feet above the bed of the sea by a buoy, in order to prevent the entrance of silt as much as possible, and it can be readily detached from the iron piping whenever it has to be cleaned.

After many difficulties, and even dangers to life, Capt. Baird's party managed to get the gauges erected and set to work, and what with the tidal observations, observations of the barometric pressure, the velocity and direction of the wind, and the amount of rainfall—for each station has been provided with means for making such observations—very valuable results may be expected.

Lieut. Gibbs' notes on the portion of the Dang Forests, in the Guzerat district, visited by him in 1874, are of great interest, and we regret that space forbids us referring to them in detail. His observations on the inhabitants of this region are of special value; he also seems to have paid considerable attention to the fauna, flora, and geology of the district.

Capt. Heaviside's lively narrative of the pendulum work in India, of his journey home, and of the operations at Kew, will also be read with interest.

Two narratives of somewhat unusual interest are given in the Appendix. One of these, by Lieut.-Col. Montgomerie, gives an account of a journey to the Namcho or Tengri Nur Lake, in Great Thibet, about ninety miles north of the Brahmaputra, by a native explorer, dur- 1871–72. The explorer was a semi-Thibetan, a young man who had been thoroughly trained for the work, and who was accompanied by four assistants. The party set out from Kumaon in November, and crossed the Brahmaputra at Shigatze, and amid considerable hardships made their way northwards, reaching the lake about the end of January, when they found it completely frozen over, although the water is so salt as to be unfit for drinking. The party intended to travel all round the lake, which is 15,200 feet above the sea, fifty miles long and from sixteen to twenty-five miles broad, and intended to proceed further to the northward and take complete surveys, but were robbed of nearly all they had, and were thus compelled to beat a rapid retreat, which they did by way of Lhasa.

During the greater part of his journey to the Namcho Lake the explorer found the streams all hard frozen, and he was consequently much struck by the number of hot springs which he met with, and more especially by the great heat of the water coming from them, his thermometer showing it to vary from 130° to 183° Fahrenheit, being generally over 150°, and often within a few degrees of the boiling point, being in one case 183° when the boiling point was 183¾°. The water generally had a sulphurous smell, and in many cases was ejected with great noise and violence; in one place the force was sufficient to throw the water up from forty to sixty feet. These springs in some respects seem to resemble the geysers of Iceland.

To the south the lake is bounded by a splendid range of snowy peaks, flanked with large glaciers, culminating in the magnificent peak "Jang Ninjinthangla," which is probably more than 25,000 feet above the sea. The range was traced for nearly 150 miles, running in a north-easterly direction. To the north of the lake the mountains were not, comparatively speaking, high, nor were there any high peaks visible further north as far as the explorer could see from a commanding point which he climbed up to. He only saw a succession of rounded hills with moderately flat ground in betwen them. Immediately north he saw a lake of about six miles in length, which he was told was called Bul Cho, from the borax (bul) which is produced there in large quantities, supplying both Lhasa and Shigatze with most of the borax that they require.

The Tengri Nur or "Namcho" Lake is considered to be a sacred place, and although at such a very great distance from habitations and so high above the sea, it boasts of several permanent monasteries and is visited by large numbers of pilgrims. There are several islands in the lake, two of them large enough for monasteries: at the time the explorer was there the Lamas on the islands kept up their communication with the shore by means of the ice, but he did not hear as to what was done in summer. Fish are said to be abundant, and modern lake shells were found on the shore as well as fossil shells, which were very numerous and of all sizes.

The narrative contains many other valuable observations made on the people and the country through which he traveled; there is a good map of the route.

The other narrative is quite equal in interest to that just referred to. It consists of extracts from a native explorer's narrative of his journey from Pitoragarh in Kumaon *via* Jumla to Tadum, and then down through Nepaul, along the Gandak River, to British territory. The explorer, who had to exercise much determination and ingenuity, took minute notes by the way of all he saw, and has added much to our knowledge of the geography, the people, and the products of a region comparatively unknown. He had to cross many rivers by the way, which was generally done by means of ropes suspended between the banks. The explorer wished to proceed much further than Tadum, which is a little beyond the Brahmaputra, in Great Thibet, but was prevented by the head man of the village. He started on July

1, 1873, and reached British territory again about the end of November, after having traveled nearly 500 miles. We have space to notice only one interesting phenomenon which he observed. At Muktinath, near Kagbeni, about 11,280 feet above the sea, in N. lat. 29° and E. long. 83° 45′, about 600 feet south of the temple, is a small mound with a little still water at its base, having a sulphurous smell. From a crevice in this mound, at the water's edge, rises a flame about a span above the surface. The people of the place told the explorer that the water sometimes increases in quantity sufficiently to flow into the crevice; the flames then disappear for a while, and there is a gurgling noise, a report, and the flames burst up and show again. This spot is called Chume Giarsa by the Bhots.

Our readers will see, from the cursory glance we have been able to take at this Report, that it contains much valuable matter apart from the immediate work of the Survey, the members of which are doing good service to India and to science.

IRON AS A CONSTRUCTIVE MATERIAL.*

From "The Architect."

It is only of late years that iron, as compared with other metals, has been used as a constructive material, but it was known and employed for various other purposes from the very earliest times; and though it is now the metal of all others the most frequently used by, and is the best adapted of any to the requirements of, the architect or engineer, it is, as I say, comparatively recently that its great value for building and constructive purposes has been fully appreciated, and, to a certain extent, utilized; and it is with the hope of showing that it may be employed in a still better manner than at present, I venture to take up your time this evening.

Though the use of iron by architects in building structures has enormously advanced, the credit of discovering and applying the great advantages that iron unquestionably possesses over almost every other material to constructive purposes, is due, I think, to the engineers and not the architects. Architects as a body have neglected and slighted this universally useful metal, either rejecting it altogether, or employing it as it were under protest, and as if they were ashamed of it; they use it in fact as a drudge, and not as I venture to think they should, as a valuable friend, equal indeed to most other building materials and superior to some; valuable both for constructive and decorative purposes, and I apply these terms in the same sense as we employ them when speaking of wood, stone, or any other material we use in building; and while it is remarkable that we should have thus neglected it, the way in which engineers seized it is no less remarkable, for they with wonderful acuteness brought their science and practical knowledge to bear upon it, producing results that ought to be an example to us; for, as a rule, engineers, with regard to brick or stone, pay us the compliment of copying as well as they can our architectural forms and practice; but with respect to iron the reverse is the case, as they, finding that architects had done, I will not say could do, little or nothing with it, struck out a path for themselves, and it cannot be denied, have achieved in it a great success. I think, however, it is unfortunate to some extent that they did so, for it is in a great measure the cause of the want of appreciation iron obtains from architects, not because architects are jealous of the success of the engineers, but rather because of the disgust they feel at the inartistic result of their labors. Can this be remedied, and can iron be placed in its proper position with regard to architecture? I venture to hope it may, by taking advantage of the practical skill and knowledge which engineers have already obtained, and upon the foundation laid by them, advancing step

* A paper read before the Royal Institute of British Architects by Mr. C. H. Driver.

by step, till we succeed in finding uses for iron both in construction and decoration, which, while perfectly adapted to the material, will yet combine and harmonize with those we have heretofore had in use.

Let us consider for a moment some of the principal attributes of iron, and then see how architects generally take advantage of them. As regards wrought iron—first, it is very strong, bearing a working tensile strength of from five to six tons, and a compressive strain of from four to five tons per inch of section, and as regards strength it is as twenty-seven to five as compared with oak, and as twenty-seven to four as compared with fir, and yet if it is employed as a beam or girder, it is generally so swaddled up with cradling and lath and plaster, that as much room is taken up by it as if it had been a beam of oak or fir. Then again it is very light as compared with its strength, but by the same process at last mentioned, its weight is brought up to that of a wood beam. It is very ductile, easily hammered to any variety of shape, and yet almost the only form ever given to a wrought iron girder when used in building, is that of the ordinary rolled or plate girder.

Again, iron, though very durable, is not an imperishable material, and this appears to be practically forgotten, for though, unlike wood and perhaps stone, it is free from internal deterioration, yet it is liable to serious destruction by rust and oxydation of its outer surfaces, a most important point considering the fact that but little excess of material is usually provided than is absolutely necessary for the required work, and therefore it would be but reasonable to suppose that when used arrangements should be made by which all parts of a girder or column could be readily inspected; but in the system in vogue the reverse is the case, for the girder is so covered and hidden up that no inspection is possible, nor can any means be taken to paint or otherwise preserve it from the inevitable destruction that must result from rust. It is almost the same as regards cast iron; it is a material admirably adapted for columns, from its fitness to bear great compressive strains, and by its very nature capable of assuming almost any form that architects may design, from a plain column to the most elaborate effort of ornamental art the mind can conceive, yet as ordinarily employed the cast-iron column is either a plain round shaft with a square cap and base-plate with gusset-pieces to strengthen their connection with the shaft, or as a story-post like a girder standing up on end; this column or story-post is often covered with lath and plaster, and appears in the glorified shape of a Doric, Ionic, or Corinthian column, with cap, &c., to match, or as is the case in most shops, it is left in its native bareness behind a plate-glass front.

I repeat that we are glad enough to make use of the strength, lightness, and adaptability of iron, but we are ashamed to acknowledge that we have employed it, and therefore cover and hide it up; and I think this arises in a great measure from the idea (a mistaken one, however) that iron does not accord with other materials, and is unsuited for architectural forms, and, therefore, if we use it (as at the present time we are almost compelled to do) we should do our best to hide it up as much as possible; and it is argued that it is necessary to lath, plaster, and case it up to satisfy the eye, as from its strength so little is required that no effect can be obtained in using it, and, therefore, it is better to cover it up with other materials to avoid the thinness and poverty of appearance that is produced when employed alone, in the same way that the flesh covering the bones produces a beautiful form, and at the same time hides a ghastly skeleton. But does the hiding up of iron by other material meet the object intended, viz., better effect? (and setting aside for a moment the principle of honesty of construction) is not the result obtained most unsatisfactory? For owing to the introduction of iron much larger spaces are bridged over without requiring columns and arches than heretofore, and hence there is produced a bareness and an apparent weakness anything but satisfactory to the eyes. As an example, I will take that most familiar one to all, the shop front; there, as a rule, we have a structure of three, four, or more stories high, with elaborate and massive architectural features, columns, cornices, pediment, &c., piled up with lavish richness, all carried apparently by a stone lintel

of twenty, thirty or forty feet span, and of an absurdly little depth in proportion to what in appearance it has to carry over a huge field of plate glass; while, as we all know, the real work of supporting the fine front is done by the wrought or cast-iron girder, which is hidden behind the stone fascia aided by cast-iron columns or story-posts, as the case may be. The effect is not pleasing or satisfactory for it is untruthful, and I contend that if the money spent upon the sham lintel that forms the casing to the girder were spent upon the girder and column by making them pleasing in design and form, the effect would not only be much better but positively good, for though we should still have the wide span and the plate glass under as before, yet we should see how the building above was really carried, and as we know that iron is strong and capable of doing its work, the eye as well as the mind would be satisfied.

With regard to this point, viz., the satisfaction of the eye, it is possible that the eye may require some amount of education before it becomes accustomed to the use of iron and its employment in connection with other material. For we are so accustomed to see beams, columns and brackets of certain proportions that we are at first sight shocked at the idea of detached columns of twenty-five or thirty diameters carrying great loads, or slender beams carrying a heavy building; and it is difficult to adjust their proportions with the styles of architecture we have in use. But I have hopes that architects will, if they give the matter their earnest attention, with the sincere desire to succeed, produce designs for iron which, though not perhaps exactly in accordance with any existing particular style, shall yet harmonize, even perhaps by contrast, with them. Iron sometimes meets with other but very different treatment from the hands of architects, and I hardly know which is the worst, for instead of being hidden, it is brought prominently forward, but then not as iron, but something else, such as stone or wood, especially so in the case of cast-iron, for not only is it made to represent the last-named, but it also appears in the guise, or rather disguise, of wrought iron. I may instance balustrades, vases, parapets, tracery, &c. A prominent example of its misuse in this way is seen in the parapet and spandrels of Westminster Bridge, though happily, however, these were not the work of an architect.

There is, I think, another reason why architects as a rule ignore iron as a constructive material, and that is perhaps the most general one, viz., few of them comparatively know anything about it, never studying or looking upon it other than as the aforesaid useful drudge, and this more especially so with respect to wrought iron, and as to cast, they may perhaps use it for columns, railings, finials, or rain-water gutters and spoutings, but these they take ready designed from an ironfounder's catalogue, and they may, or which is more often the case, may not harmonize with the rest of their design, they thinking it is not worth their while to take the trouble to design such things for themselves. Or if they want a wrought-iron girder, they are, perhaps, able to work one out from the simple formulæ given in the various handbooks; or, as is more likely, they leave it to the builder's foreman. But if the quantity required is large, and the work important, they then employ an engineer to work out the calculations, and as the engineer (with every respect to him) cares nothing about art, but a great deal as to whether his girders are strong and economical, it is very probable that the resultant work is ugly, and as without doubt the ordinary plate girders and columns, used in buildings generally, are ugly, the architect naturally enough covers them up with a material he does know something about, and therefore can design in; but if the architect did know and understand as much about iron he would calculate for himself, and study to so design his girders or columns, or whatever else he may require, that the result should be artistic and suitable to the structure for which it was intended.

Surely architects, if they will, can so design their girders in wrought or cast iron that they shall be pleasing and effective. Let them but take the trouble to draw them out and calculate them for themselves, they will soon find it easy enough to arrange flanges, webs, cover plates, angle and tee irons so symmetrically as to be pleasing, and still preserve

the necessary scientific proportions and the relation of the several parts to each other in a practical manner—plates and angle and tee irons are now rolled in such length that very large spaces may be spanned by girders without any cover or junction plates being required. As for instance, plates can be obtained from 20 to 25 feet long by 2 to 3 feet wide; angle and tee irons up to 30 or 35 feet or even 40 feet. Many varied forms and even mouldings could and would be rolled, if manufacturers found there was a demand for them, and that it would pay to make the necessary rolls.

Reverting for a moment to the point that the constructive employment of iron is of comparatively late date, it is worthy of remark the significant fact that the artists of the Middle Ages had brick and stone and other materials, but no iron—at least not in quantities they could make structural use of, and they made such good use of the materials they had that we are feign to copy them. Is it not therefore fair to suppose that if they had had iron at their command as we have, they would have produced works in that material as admirable as are their works in others? and I am justified in assuming this from the wonderfully beautiful works they achieved in the ornamental wrought-iron work they did make. I cannot help, therefore, feeling that, to a certain extent, the poor results we have accomplished with all the facilities we have at our command is not a cheering instance of the progress of true art in these modern times.

There is yet another matter closely connected with iron as a constructive material which requires attention, and that is the relative positions in which wrought and cast iron should be placed, viz., whether in internal or external work, and this more especially applies to ornament. Now it is a certain and well-known fact that wrought iron is much more susceptible to the influence of weather as regards oxydation than cast, and though, therefore, there can be no question as to the superior art and beauty of wrought iron, yet it is a matter worthy of some consideration, if it be not more advisable, for the sake of durability, to employ cast iron for ornamental work externally, and confine our use of wrought iron to purposes of internal decoration. I am perfectly aware that in advocating the use of cast iron ornament at all I am touching upon dangerous ground, as I know that among many of the highest authorities there is a strong feeling against it, but be this as it may, the fact remains the same that cast iron is better adapted for external work than wrought, and I am inclined to think that the feeling which undoubtedly does exist against it is due to the way in which it is misused, and that if the design is properly adapted to the material one of the principal objections to its application is removed. I know it is said that cast iron ornament is inartistic, showing no feeling, utterly wanting in individuality, and vulgar in the extreme, so that cast iron ornament has almost become a by-word; but surely it is unfairly treated, for might not the same be said of work in bronze? A work in cast iron requires to have a model prepared and a mould made, so also does a work in bronze. The iron has to be melted and run into the mould, and it is the same with bronze; if the model is badly designed and badly executed in either case, the resultant cast will be bad also.

With respect to iron as a constructive material, the different qualities of the metal used is a very important and serious point, much more so than at first sight appears; for, as in the case of cast iron, there is not only a great difference of strength in the different brands, but also in the same iron, from the manner in which it is manufactured, and it is almost impossible to judge by the outward appearance of a casting whether the iron used is good or bad, for even when fractured it requires great skill and experience to do so. I do not, however, purpose to go into this matter this evening.

Hitherto I have only spoken of matters which concern iron as a building material, but I propose, with your permission, before closing my Paper, to add a few remarks upon constructive ornamentation of ironwork, or, as it would perhaps be better to put it, the ornamental construction of ironwork; for, though in my previous remarks, I have several times referred to ornamental work in iron, it has been irrespective of its being constructive or otherwise. I can, however, only give a passing glance

at it, for the subject is one which in itself would extend to almost any length.

We most of us know what ornamental construction consists of in wood or stone as opposed to constructing for ornament, but it is, I confess, difficult to apply the principles which guide us in the last-named materials to iron; for though it is true we can, as I have said, so arrange our tee and angle irons, webs and plates, &c., that they shall be symmetrical, that is not all that is required, for true ornament does not consist in symmetry alone, though symmetry is a very important element in it. We are placed in this difficulty, that almost any ornament we employ on constructive ironwork has to be itself constructed, thus flying in the face of that golden rule of ornament which tells us to "ornament our construction and not to construct for ornament." When working with wood and stone and some other building materials we can build in blocks or masses of material, and cut and carve them as it seemeth to us best, and it can hardly be said that we are able to do this in the same sense in iron; but though we cannot carve it, we can stamp, emboss, engrave, and even mould it if we will, for machinery is now so powerful that mouldings, splays, chamfers, &c., can be executed in this material with nearly the same facility as in wood; and there is some ground for consolation in the fact that whatever difficulties we may have to encounter with respect to having to construct for ornament in iron, the same difficulty has to be met with in respect to all other metals, and I am inclined to take advantage of "there being no rule without an exception," and make that exception in favor of iron and all other metals; but though we may have in some measure to construct our ornament, I think we should be careful to so manage it that the ornament we do employ shall not be wholly useless, and that if it does not add much to the strength of the structure it shall not at least be detrimental, and, therefore, all added ornament in ironwork should I think be of the very lightest description, and if not actually constructive, it should at least grow naturally from, and appear to be part of, the real constructive portion of the work.

Time, however, will not permit to go further into this point, which is in itself a sufficient subject for a paper, which at some future time I may ask to be allowed to read.

Allow me, in conclusion, to thank you for your attention, and at the same time to request your kind indulgence for much that I have said. Many of you, as I know, have already by your works anticipated my ideas with respect to constructive and architectural ironwork; and to you, therefore, my remarks, I fear, have been tedious. But still, I hope, you will endorse my views, as I have been encouraged to maintain them by the knowledge that, among those who stand the highest in our profession, there are some who have not thought it beneath them to design in iron, and with successful results—pardon me, if I mention the name of one, our honored President, Sir George Gilbert Scott.

REPORTS OF ENGINEERING SOCIETIES.

THE NEW YORK SOCIETY OF PRACTICAL ENGINEERING held its third quarterly session for the year 1875, in Cooper Union, on the evenings of September 7th, 8th, 9th and 10th. The President, James A. Whitney, delivered the annual address on the evening first named. Subject—"The relation of Patent Laws to American Agriculture, Arts, and Industries." At the subsequent meetings other elaborate papers were read; on the "Minor Economies of Manufacturers," by James C. Bayles, editor of the *Iron Age*; on the "Industrial Uses of Blast Furnace Slag," noticed elsewhere in our columns, by Frederick A. Luckenbach, M. E.; on "Steam Propulsion on Canals," by George Ed. Harding. Briefer essays were also read; on "Stationary Fire Extinguisher Pipes," by Henry Palmieri, M. E.; and on the "Testing of Water Pipes and Mains," by Ernst Bilhuber, M. E. The President's address has been published in pamphlet form by the Society. The next session will be held the latter part of November next.

INSTITUTION OF CIVIL ENGINEERS. — The council of the Institution of Civil Engineers have awarded the following premiums:—Telford medals and Telford premiums to the following gentlemen:—to Mr. W. Hackney, for his paper on "The Manufacture of Steel;" Mr. H. E. Jones, for his paper on "The Construction of Gasworks;" Mr. A. R. Binnie, for his paper on "The Nagpur Waterworks;" Mr. G. F. Deacon, for his paper "On the System of Constant and Intermittent Water Supply, and the Prevention of Waste;" Telford premiums to M. J. Gaudard, of Lausanne, for his "Notes on the Consolidation of Earthworks;" to Professor Prestwich, for his paper "On the Origin of the Chesil Bank;" to Mr.

J. T. Smith, for his paper "On Bessemer Steel Rails;" to Mr. C. Colson, for his "Details of the Working Tests and Observations on Portland Cement;" to Mr. T. C. Watson, for his "Description of the Use of Facines in the Public Works of Holland;" a Watt medal and the Manby Premium, to Mr. J. C. Hawkshaw, for his paper on "The Construction of the Albert Dock at Kingston-upon-Hull." The Council have likewise awarded the following prizes to students of the Institution:—Miller prizes to the following gentlemen:—Mr. A. E. Baldwin, for his paper on "The Design and Construction of Lock Gates;" Mr. J. C. Inglis, for his paper "Experiments on Current Meters and their Bearing on the Hydraulics of Rivers;" to Mr. W. B. Myers, for his "Comparison of the various forms of Girder Bridges, showing the Advantages of the Schwedler Bridge; together with an elucidation of the Theoretical Principles of the same;" Mr. A. S. Moss, for his paper on "The River Humber;" Mr. W. P. Orchard, for his paper on "Hydraulic Calculations relating to Water Pressure and Walls to resist it, Gauging of Water, the Flow of Water in open Channels and in Pipes;" Mr. J. Tysoe, for his paper on "The Manufacture of Illuminating Gas from Coal;" Mr. J. C. Mackay, for his paper on "Concrete." The following note has also been issued by the Council:—"It has frequently occurred that in papers which have been considered deserving of being read and published, and have even had premiums awarded to them, the authors may have advanced somewhat doubtful theories, or may have arrived at conclusions at variance with received opinions. The Council would, therefore, emphatically repeat, that the institution must not, as a body, be considered responsible for the facts and opinions advanced in the papers or in the consequent discussions; and it must be understood that such papers may have medals and premiums awarded to them, on account of the science, talent, or industry displayed in the consideration of the subject, and for the good which may be expected to result from the discussion and the inquiry; but that such notice, or award, must not be considered as any expression of opinion, on the part of the institution, of the correctness of any of the views entertained by the authors of the papers."—*Engineer.*

IRON AND STEEL NOTES.

A German paper states that the steel works of Frederick Krupp, of Essen, are about to receive a very important addition to their machinery. The largest steam-hammer at use at these works at the present time is one capable of working a mass of steel 50 tons in weight, and erected at a cost of 2,800,000 francs. It is now in contemplation to build a new steam-hammer capable of beating up a mass of steel of double the weight, viz., 100 tons. The new machine, it is estimated, will cost 5,000,000 francs, and will be the most powerful in the world, and it may be expected that the size and weight of the German artillery will be enormously increased.—*Engineer.*

Purifying Iron.—Mr. Wm. Baker, of Willenhall, employs a vessel or trough placed between the furnace and the moulds or other receiver for the molten metal, and forms the vessel or trough preferably oblong and a few inches deep on one side and shelving up to the top on the other side. He closes the end of the vessel or trough to retain the metal to be acted upon, and forms an opening in the top of each end, one for the admission and the other for the discharge of the molten metals. He forces air through tuyeres placed along that side of the vessel or trough to which the bottom shelves up, and inclines the tuyeres towards the surface of the metal with their nozzles nearly touching the metal, so that the air will be forced into and through the metal. He carries up the sides of the vessel or trough and covers the top with a perforated plate. The metal flows through the vessel or trough and is purified by the action of the injected air.—*Mining Journal.*

Cast Iron Chilled Wheels for Carriages.—A number of gentlemen interested in railways, engineers and others, met at the machine works of Mr. Horn, Millbank Row, Westminster, lately, for the purpose of witnessing the results of tests applied to the "cast iron chilled wheels" manufactured by Barnum, Richardson & Co., of the Salisbury Ironworks, Connecticut. It was stated that these wheels have been in use for a long time both in the United States and Canada on almost all the railways of these countries, with the result that on some lines they are now used to the exclusion of all others. The experience of America, where the frost is so severe, would, therefore, seem to be in favor of these wheels, but as an opinion existed in England that they were easily fractured, the manufacturers resolved to try the question by experiment, and hence the appeal to the tests applied. These were certainly of a severe kind, and it was not until the wheels had been struck 267 times with two hammers weighing 28 lbs. and 32 lbs. respectively, that the iron partially gave way. It is claimed for the wheels that they are not only the most safe, but the most durable and economical—*London Mining Journal.*

Dephosphorization of Iron Ores.—The following process for effecting the dephosphorization of iron ores has been patented by its inventor, M. G. Velge, of Liege:—When a substance containing phosphate of iron is fused with two or three times its weight of a mixture of carbonate of soda and potash, the phosphorus can be removed in the form of alkaline phosphate by washing. Although this process is applicable to the treatment of small quantities only, its principle is that upon which M. Velge bases his own. He found (1) that chloride of sodium can be substituted for these carbonates; (2) that it is sufficient to add to the ore a weight of this reagent only a trifle in excess of the phosphate contained in the substance—say 6 parts of salt to 5 of *phosphate*, or 1 lb. of salt to about 1-5th lb. of *phosphorus*; (3) when the mixture has been well made the ore should not be fused, but kept for some time at a mere red heat. When the gases

have all been given off, water slightly acidulated with hydrochloric acid is added, and the phosphate dissolves after a little time. At first he used to crush the ore and the salt together, but, beside the expense of the operation compared with the low cost of the matter operated on, the final result was unsatisfactory. The ore came out in powder, with which there was every chance of choking the blast. He then proceeded to dry strongly, or slightly calcine, poros ores, adding to them a concentrated solution of sea-salt. This solution was taken up very greedily by the roasted ores, some varieties absorbing as much as 40 per cent. of their weight. In this way all the molecules of phosphorus are brought into the presence of the salt. After calcination and successive washings the quantity of phosphorus held by the ore was reduced from 1.25 per cent. to less than one two-thousandth. Practically, perhaps, so high a degree of perfection would hardly be arrived at, but it it is contended by the inventor that the process itself is quite satisfactory. Four operations are involved in the dephosphorization :

(1.) The desiccation of the ore by waste heat or other suitable method. If there be much phosphorus to remove, it will be best only to use such ores as lose much water on drying. On the other hand, if the ores contain but little phosphorus, it will be useless to dry them.

(2.) The absorption of a solution of salt, stronger or weaker, according to the proportion of phosphorus.

(3.) *Calcining.*—In the ordinary way the gases of the blast-furnace are available for calcining, and when this is the case, the calcining can be effected in a vertical oven, the gases being kindled from below. In the absence of such gases, a reverberatory furnace must be employed, for the calcining in a vertical furnace by admixture of coal has the effect of partly reducing and melting the ore, and thus rendering the washing almost impossible. Not only are the pores of the ore choked in part, but, in the case of silicious ores, the phosphate of soda is converted into silicate of soda. In making use of the blast furnace gases all the carbonic oxyde is consumed before reaching the ore, and there is no sign of reduction, even at the brightest red heat.

(4.) *Washing.*—The ore should be left for several days in vessels filled with water, taking care to renew the water frequently, and to add at each renewal a small quantity of hydrochloric acid. The water by itself would have but a small effect upon the phosphate. It is of the greatest importance to conduct the washing with care, for the success of the operation depends upon it.—*Iron.*

UTILIZATION OF SLAG—ADDRESS BY F. A. LUCKENBACH BEFORE THE SOCIETY OF ENGINEERING.—Frederick A. Luckenbach addressed the New York Society of Practical Engineering at their last session. The speaker's topic was "The Industrial Uses of Blast Furnace Slag," and was illustrated by the exhibition of specimens. The subject was further discussed by Prof. Whitney, the President of the Society, and by Messrs. Moore, Sutton, Roosevelt, and others. The main points of Mr. Luckenbach's paper are as follows :

Slag is a chemical compound, the combination of an acid with various bases, and is as much a salt as the sulphate of alumina or potassa. Its formation is strictly governed by the laws of chemistry. The silica is the acid, and the lime, alumina, magnesia and the alkalies are the basis. Iron ores are generally silicious. If when an ore is placed in a blast furnace and smelted no base is added, in seeking a base the ore will seize on the oxyde of iron, combine with it and carry it off as slag. To prevent this, limestone, which is a base, is added. A certain quantity of silica requires a certain amount of lime to saturate, another quantity of magnesia, and another of alumina; all of which quantities will vary with their chemical equivalents. Having then analyses of all the material of the charge, the proportions of each may be so calculated as to produce a certain slag. But with slag, as with other chemical compounds, there may be two atoms of base to one of acid, two of acid to one of base, or one of base to one of acid; and, according as this is the case, they are called basic, acid, or neutral slags. The acid slags are the most fusible, the neutral next, and the basic the least so. Slag is sometimes, according to the proportions of its component parts, a material easily fusible, and possessed of other definite qualities, and at other times a comparatively infusible material. Such being the character of the product, it has been a problem of great difficulty to determine what general system can be hit upon which for any given purpose will utilize all the different varieties of slag.

The first recorded plan for the utilization of slag was that of John Payne in England, in 1728. He proposed molding the dross by fusing or melting with such mixtures as will prevent its being brittle, and also give it different colors, so as to make it more ornamental and useful. After this came Moshet's plan of 1815, for reworking slag to obtain the iron left in it. Then Crawshay and Moshet invented a process for recovering the iron believed to exist in the refuse of copper smelting, which process appears to be the first use of water for pulverizing molten slag. In 1852, Alexander Cunningham claimed that sulphate of alumina and alum could be obtained from slag. In 1853, William and John Longmand thought blast-furnace slag could be formed into shapes suitable for pavements of streets. In the same year George Robinson proposed a new plan. The slag was to be run in a molten state upon a heated iron table and formed into sheets by rolling. The plates were then to be annealed and applied for roofing and other purposes. In the following year Smith, Bessemer & Longsdon secured a patent on a process in which slag was to be turned into table tops, chimney pieces, statues, etc. Joseph Woodard of Yorkshire, patented a process for making bricks of slag for building purposes. A company of capitalists have lately began to make bricks by this process.

Their prospectus asserts that the brick will withstand a crushing force of over four tons per cubic inch, being five times more than ordinary brick will bear. All these projects have proved failures. The secret of securing homogeneity in structure, irrespective of chemical composition, was not discovered, although the practice of annealing gave a faint and shadowy hint of the direction in which it might be found. Mr. Luckenbach read the details of an invention of his own, which he claimed to be an improvement in the means of annealing castings made of blast furnace slag, and which is designed to provide for the manufacture of paving and building-blocks, fire-brick, and other articles from the slag.

RAILWAY NOTES.

IMPROVEMENTS IN TRAMWAYS.—The invention of Messrs. NIEMANN and GEIGER, of Vienna, consists in laying the rails of tramways on a number of supports or chairs made by preference of cast metal, and if of metal they are made hollow in the shape of an open box, and bridged over on the top with a recess to receive the rail, so that the upper surface of box and rail is practically level. The box may be filled in with any suitable material. For fixing the rails to the chairs small keys or wedges are driven into grooves formed on one side of the recess, pressing the rail against the other side of recess. To keep the gauge, tie rods may be used.

FAST RAILWAY TRAVEL.—With the improvements made by all the principal American railroads in the last few years, by which a perfection of track has been secured, equal, perhaps, to any in the world, the speed of our express trains is not only rivaling the best time of England, but in some respects surpasses the grandest achievements of travel attained in the mother country. It is no unusual thing for trains in the United States to run at a speed exceeding thirty miles an hour for long continuous distances, and even this rate is considered slow on some of the main lines. As an instance of what can be done by our roads, it is well to state a recent occurrence. Some two weeks ago, a special train passed westward over the Pennsylvania Railroad, carrying an excursion of eastern editors and *literati* on their way to California. The train ran from Harrisburg to Altoona, a distance of 132 miles, in exactly three hours, without stopping. So regular was the speed, and so smooth the track, that scarcely any of the party could realize the fact that they had been traveling through the mountains of Pennsylvania at the rate of forty-four miles an hour, when the time and distance were made known over the dinner table at the foot of the Alleghenies.—*The Railway World.*

A RAILROAD THREE HUNDRED FEET ABOVE A CITY.—It is difficult to imagine anything better adapted to produce a vivid and startling impression on the memory than the first sight of Morlaix, Brittainy, as approached by rail. The city lies on both sides a deep, narrow valley and the railroad springs across the chasm on a magnificent viaduct 300 feet high. Entirely unprepared for anything of the sort, the traveler finds himself taking a bird's eye view of a city of the middle ages. There it lies, 300 feet below, almost as if it were in the days when Mary, Queen of Scots, passed through on her way to Holyrood and the scaffold. The precipitous, winding, narrow, darksome streets, the peaked roofs, misshapen by time and studded with curious dormer windows, are still there as when she looked upon them centuries ago, when with brilliant pageant she and her cortege of knights and ladies swept through Morlaix with laughter and song. Should it be a festal day or a fair, the sight is still more unique, for the square is then crowded with booths and peasants in various costumes, and it is positively white with the starched caps of the women. The city is divided by the river of Morlaix, an estuary up which ships come into the heart of the town. The banks of the river are faced with granite, and affords a fine promenade on each side. A smaller stream dashes roaring down the streets, bringing to the dirty lanes of the crowded town the music of the pure fountains whence it came.—*Railway Review.*

THE NARROW GAUGE IN SWITZERLAND.—The first Swiss narrow gauge line, opened in June, 1874, runs from Lausanne (on Lake Geneva), to Echallens, and is of a length of about 9¼ miles. The gauge is one metre. This line, which is now being extended to La Sarraz, 7¼ miles beyond Echallens, is laid partly into the turnpike road; the maximum gradients are 1 in 25, and the smallest curves have a radius of three chains. The rolling stock, which has been acquired, together with the rails, from the original Mont Cenis mid-rail line, consists of two locomotives, twelve passenger carriages, five luggage vans, and twenty-one goods wagons; the two locomotives however, have since been replaced by tank engines bought at the Creusot works, and there has also been added a small tank engine from Krauss and Co., of Munich. Including rolling stock, the lines has cost but £5,000. per mile. The working speed averages 12 miles an hour, and although the goods traffic is small, the undertaking has proved a very profitable one; this being due, besides the modest amount of capital engaged, to a very simple mode of management.

The Rigi metre-gauge road, a portion of which was already open last season, has been finally opened in June last. The maximun gradients (worked by the adhesion of tank engines of 20 tons weight) are 1 in 20. The rolling stock consists, besides the three tank engines, but of three passenger carriages and of three open goods wagons. The carriages on two four-wheeled bogies have each of them accommodation for 55 passengers, there being eleven parallel transverse benches, each of which is adjacent to a side door. No tourist climbing up the Rigi by either of the two rack railways, should omit a ride on this most interesting "little wonder" of a narrow gauge line, only about four miles in length, but showing along

its serpertine course from Kaltbad to the Scheideck an ever-varying panorama of the most picturesque Alpine world. At the latter station the line reaches an altitude of 1648 metres, or a little over one mile above sea level; this is therefore actually the highest railway in Europe. Like the other Rigi railways, the narrow gauge road is of course only open from May to October, that is to say for about six months in the year.

We next come to the undertaking of the Swiss Society for Narrow Gauge Railways under the direction of President Dr. Dubs. This society, which was founded in September, 1872, by some of the leading Swiss bankers and engineers, obtained in the course of 1873 concessions for the building and working of about 80 miles of metre-gauge railways, situated in different Swiss cantons. But owing to the great and protracted financial crisis of 1874, the society was unfortunately obliged to postpone the execution of the greater portion of these projects, and, in fact, only the metre-gauge railway in the Canton of Appenzel was proceeded with, has a length of sixteen miles, and the class of rolling stock adopted has been fully illustrated and descrrbrd by us (*vide* page 489 of our last volume and page 29 of the present volume). Including everything, this line—established on a very difficult ground—has cost £9,600 per mile; but a line of the normal gauge of 4 ft. 8½ in. would have cost, according to careful estimate, at the least £25,600 per mile.

Among the narrow gauge lines now being executed in Switzerland, the longest will be that from Geneva to Lausanne along the Jura Mountains. Including the lake branches to Nyon and Morges, the length of this line will be 55 miles; the gauge being likewise one metre. This line is being constructed by a local board, who will receive from the Canton of Vaud a subvention of £70,000. There are other narrow gauge projects in the south of Switzerland, regarding which, however, we have at the present moment no precise data; but counting now the lines enumerated above as either opened, building, or concessioned, there will be—at the end of another year—a Swiss narrow gauge *reseau* to a total extent of about 156 miles in operation.

Finally, we have to notice here the narrow gauge tramways projected by the well-known Swiss locomotive engineer, Mr. A. Brunner. These are to be worked by two-storied motive power cars, and a concession has been granted for such a line running from Zurich to some suburbs of that town. In connection with this interesting subject, we must not omit to point out that this progress of narrow gauge railways in Switzerland, as well indeed of their extension throughout the world, is in the largest measure due to the untiring energy of Mr. R. F. Fairlie. It was chiefly by means of his writings, which have been translated into most modern languages, that the advantages of the system he may be said to have inaugurated were thoroughly understood.—*Engineering.*

ENGINEERING STRUCTURES.

THE TUNIS EXPEDITION.—News from the Tunis Expedition has been received to the 15th ult. The Marquis Antinori and Captain Barattieri have visited Gerba Island. The explorers on the southern coast of the Gulf of Gabes sought for the ancient canal connecting Syrtes Minor with Palus Tritonia, but found none. The engineers have returned from the Palustral Basin. They examined the eastern shores. The result of their observations will be published shortly. The heat has caused the wild animals and birds to disappear, but specimens of fossils, stone utensils, and weapons were abundant. The health and spirits of the party were excellent. Colonel Galvagni has collected interesting ethnographical and statistical details.

THE SUEZ CANAL.—The opening of this isthmus was supposed at one time to restore to the Mediterranean ports of France, and especially Marseilles, their old splendor, instead of which most of the steamers prefer to proceed direct through the Straits of Gibraltar, thus leaving the French railways to their own greediness. Nor has the French merchant navy been in due proportion benefited by the opening of the new sea-road to the far East. The French Shipping passing through the canal decreases, relatively speaking, from year to year. In 1870, 436,000 tons burthen passed through the canal, of which 289,000 were English, and one-fifth, or 84,000 only, French. In 1874, the proportion of French bottoms is found to be reduced to 220,000 tons, or less than one-tenth, 2,423,000 tons having passed the canal, out of which 1,797,000 were under the English flag. The other navies, although below the absolute figure for France, are progressing more rapidly. Thus, since 1872, four times more Dutch ships used the canal, while the increase of the French navigation was only 40 per cent.

THE MISSISSIPPI IMPROVEMENTS.—The Board of Engineers, which assembled for the purpose of considering the plans of Captain Eads for the improvement of the Mississippi, have concluded their labors for the present.

The following gentlemen constituted the Board at its recent session: Gen. Barnard, President; Sir Chas. A. Hartley, Gen. Alexander, Messrs. Roberts, Whitcomb, and Sickles. After considerable discussion, the Board agreed unanimously upon the following report:

I. With regard to the priority of construction of different parts of the work, the Board recommended that the seats of both jetties and of the spar joining the west jetty with the right bank be protected with mattresses throughout this entire length—that is, that first of all the foundation of the east jetty be secured out to a depth of 30 feet, and of the west jetty to 20 feet. They further recommend that the east jetty be carried up the water line before raising the mattress wall of the west jetty to the same level, and that the construction details of the pier-heads be left

till the Commission can meet at the jetties this Fall.

II. After attentive examination of the plan of construction, consisting of a combination of willow mattresses and stone, now in execution by Mr. Eads, the Board find it to be a modification of methods long in use in Holland and elsewhere. It is essentially the same as that applied to the jetties of the mouth of the Oder, and also to the jetties at the new mouth of the Maas, so satisfactorily as to draw from the legislative body of Holland the expression that "their complete success has removed all doubts as to the possibility of making piers at sea on our coast." It is moreover essentially the same as that adopted by the recent Commission (1874) for these works.

III. The Board advise that Bayou Grande be left open for the present.

The same Commission will reassemble at the mouth of the Mississippi during the latter part of October or the first days of November.

THE ST. GOTHARD TUNNEL.—The international Commissioners, whose duty it is to inspect and report upon the progress made with the St. Gothard Tunnel and railway, have this year required a more detailed statement of the work executed than has been hitherto furnished. This statement appears in a tabular form accompanied with explanations and remarks in the *Politecnico*, a scientific journal published at Milan. From it we learn that during the last three months before the publication of the report there had been excavated at the Goenechen or Swiss side of the mountain 341.3 metres, being at the rate of 3.71 metres per day, and on the Italian side at Acrolo, 184 metres, or at the rate of only 2 metres per day, which together would give a progress at the rate of 2,100 metres per annum, the comparatively slow progress made on the Italian side being due to the hard nature of the rock met with, through which fortunately, however, little water percolated, so that the work was not impeded by that hitherto prevailing obstacle. With reference to the masonry and the excavation of the tunnel to its full dimensions—these are (says the report) evidently proceeding so slowly that series embarrassment is likely to result from having too great a length of the small tunnel or drift way excavated in advance of the completed work. The number of perforators in operation, worked by compressed air, are stated to be sixteen in number, the compression of the air being effected by water power. We are left uninformed as to the length of the tunnel actually completed, as opposite the heading in the tabular statement "Length of Tunnel Completed," no figures appear. As regards that portion of the St. Gothard railway which follows the valley of the river Ticino, the works appear to have been prosecuted with considerable energy, as trains have been running on the sections Lugano-Chiasso and Biasco and Biasco-Bellinzona ever since December 6 last, or exactly three years since the formation of the St. Gothard Railway Company. As regards the section Bellinzona-Locarno, it has not been opened in consequence of the damage done by floods, which had rendered it impossible to construct the iron bridge at Verzasca. There are still incomplete in two other sections of the line many complementary works, and in some instances the trains have to pass through tunnels in which the centres and supports still remain. These lines have been opened rather in compliance with a stringent clause in the concession than that they can be considered in a fit state for traffic.

ORDNANCE AND NAVAL.

FIELD ARTILLERY EXPERIMENTS AT DARTMOOR.—The series of trials of the effect of the fire of our service horse artillery and field artillery on broken ground representing the conditions of actual warfare, is now in progress. The principal objects are the trial of the relative effects of shrapnel shell with time and percussion fuzes, of common shell burst with powder, and also with gun cotton with the surrounding space filled with water, and the cotton fired by a detonator; the efficiency of Capt. Nolan's range finder, as compared with the employment of individual judgment and trial shots to ascertain the distance of an enemy. It is proposed to review the result of these experiments when the series is completed.—*Engineer*

THE DEUTSCHLAND.—This iron-armored frigate, the sister ship to the Kaiser, built for the Imperial German Government by Messrs. Samuda Brothers, from the design of Mr. E. J. Reed, is now almost ready to be handed over. She is at present in the Millwall Docks. It is said that there is no dry dock on the Thames large enough to hold her. Though her length is only 280ft., her beam is 63ft.; still we should have thought that she could have been accommodated either in Messrs. Lewis and Stockerill's dry dock, or at the Thames Ironworks. Perhaps, however the German Government like to save a few pounds as much as any one else. The magnificent work put into her both by Messrs. Samuda and by Messrs. Penn, who have supplied the engines, deserves the highest commendation. Indeed, the engines are a picture to feast the eye upon—their compactness and finish being so admirable. We notice that in these two frigates Messrs. Penn have introduced some improvements which are not to be found in any of their previous engines. Formerly the screw-shaft worked upon only three bearings, with the turning-wheel at the after end of the engine room, situated about the middle of a length of shaft between two bearings some 11ft. apart, by which great vibration was caused. Now a fourth bearing has been introduced, and the turning-wheel placed in the centre of the engine-room, by which means great steadiness ensues. This arrangement also admits of the shaft being made in two pieces, and coupled in the centre. The Kaiser and Deutschland are also provided with steam starting-gear, which we do not remember having seen before fitted by Messrs. Penn to trunk-engines. The Deutschland is complete with the exception of her guns, of which she

is to carry eight 26in. guns of 22 tons each, and one 22in. gun of 18 tons. These are to be supplied in Germany, by Messrs. Krupp, of Essen, who will also fit the racers for them. —*Engineer.*

BOOK NOTICES.

HOW TO TEACH CHEMISTRY. By EDWARD FRANKLAND, F. R. S. London: J. & A. Churchill. For sale by D. Van Nostrand. Price $1.25.

This is simply a condensed report of six lectures delivered by Dr. Frankland, and carefully summarized by one of the science teachers at South Kensington.

The suggestions afforded to teachers are of the highest value, not only as to the order of subjects, but in reference to the manipulation of apparatus in illustrating the science.

The diagrams are numerous and excellent.

NOTES ON CERTAIN EXPLOSIVE AGENTS. By WALTER N. HILL, S. B. Boston: John Allyn. For sale by D. Van Nostrand. Price $1.00.

This work is in the form of a pamphlet of seventy pages; but within this space is included an epitome of the present knowledge of all the explosive agents at present in use.

The topics treated in separate chapters are: I. Explosions and Explosive Bodies; II. Nitro-Glycerine; III. Gun Cotton; IV. Picrates and Fulminates; V. Classes of Explosive Mixtures; VI. Use of Nitro-Glycerine and Gun Cotton.

Some folding plates illustrated the manufacture of Nitro-Glycerine.

A DICTIONARY OF CHEMISTRY. By HENRY WATTS, B. A., F. R. S. London: Longmans, Green & Co. Second Supplement. For sale by D. Van Nostrand. Price $15.00.

The Second Supplement to this well known work completes the record of chemical discovery down to 1873, and contains the more important advances in science made in 1874.

The volume is quite as large as either of the others.

The leading contributors, with the list of their contributions, are given herewith:

H. E. Armstrong, F. C. S.—Phenols—Sulphur Chlorides.

G. C. Foster, B. A., F. R. S.—Magnetism.

H. E. Roscoe, F. R. S.—Chemical Action of Light, Spectral Analyses.

Robert Warrington, Esq., F. C. S.—Fodder, Maize, Malt, Oats, Root Crops.

THE MECHANIC'S FRIEND; A COLLECTION OF RECEIPTS AND PRACTICAL SUGGESTIONS. With numerous Diagrams and Woodcuts. Edited by WM. E. A. AXON, F. S. S. New York: D. Van Nostrand. Price $1.50.

This convenient little volume is made up of those applications of Physics and Chemistry, with which amateurs chiefly delight to deal.

Most of the matter has appeared in the columns of the *English Mechanic* during the past two or three years, such selections having been made from those columns as seemed most valued by the readers.

Upon such subjects as the following there are several articles by as many different writers: Bronzing, Cements, Dyes, Electricity, Gilding, Glass Working, Glues, Horology, Lacquers, Locomotives, Magnetism, Metal Working, Photography, Pyrotechny, Solders, Steam Engines, Telegraphy, Taxidermy, Varnishes and Water Proofing.

Perhaps the more interesting portions of the book to the mechanic will be those relating to Tools, Locks, and Special Processes in Mechanical Engineering and Chemistry.

The illustrations are all good.

RUDIMENTS OF GEOLOGY. By SAMUEL SHARP, F. S. A., F. G. S. London: E. Stanford, 1875. Price $1.75.

The introductory portion of this little book was originally prepared for use in the writer's class, and is now published, with large additions, for the benefit of persons similarly circumstanced, and of private students. Neither could desire a more useful help, for we know of no book in which the principles and facts of geology are so well epitomized, or in which either are stated in such a clear and popular manner. The introductory part deals with the generalities of the subject, its divisions, the materials of the earth's crust, and the manner in which these have been formed and modified, all of which is presented in such order as to be both easily comprehended and remembered by the learner. The second part is stratigraphical and paleontological, and in it the different formations are described in ascending order, and their construction and characteristic fossils indicated. Much that is important in the philosophy of the science is also communicated in this division, and the work as a whole may be honestly recommended to educators and self-educators alike as a cheap and reliable handbook.

HYDROLOGY OF SOUTH AFRICA; OR DETAILS OF THE FORMER HYDROGRAPHIC CONDITION OF THE CAPE OF GOOD HOPE, AND OF CAUSES OF ITS PRESENT ARIDITY. Compiled by JOHN CROUMBIE BROWN, LL. D. Kirkcaldy: J. Crawford.

The above title explains fully the scope of the work. It is a valuable contribution to the science of Physical Geography. The practical bearings of the subject are not at first apparent, but are none the less real. The decrease or increase of rainfall in different sections of our country is a subject upon which we have no definite knowledge. Such changes are so slow that much time is required to gather data enough to establish the fact of any permanent change. In the case of South Africa, the writer finds that a close study of its topography yields much information of value.

The contents, as given by chapters, are as follows: Testimony supplied by the Physical Geography of South Africa; Testimony in regard to former condition of South Africa supplied by Geology; Indications of former Hydrographic Conditions; Hydrographic Condition within the Historic Period; Primary Cause of Desiccation of South Africa; Secondary Causes; Aridity and Water Supply beyond the Colonized portions of the Country; Water Supply within the Colony.

The writer has availed himself of the testimony of standard authorities, and extracts from former writings are quite abundant. There are no maps nor illustrations of any kind.

PRACTICAL GEOMETRY AND ENGINEERING DRAWING. By G. SYDENHAM CLARKE, R. E. London, 1875.

Lieut. Clarke is instructor in geometrical drawing at Cooper's Hill College, and a few words from his preface will best explain the origin as well as the plan of his book. In dealing with large numbers of students, the writer felt the want of "a text-book which explained first principles fully and systematically, which preserved a clear and logical sequence throughout its pages, and which furnished examples bearing directly on the subject-matter of each chapter. Existing works did not satisfactorily meet the case. Some supposed the student to know too much, others gave him credit for knowing nothing. . . . . The objects of the writer have been to bring general principles into prominence, to illustrate those principles by a variety of problems fully explained, pointing out at the same time any peculiarities worthy of remark; and finally, to append to each chapter a number of problems with occasional hints as to their solution." The plan thus sketched out is systematically adhered to, each chapter in the section on solid geometry starting from principles and definitions, going through explanations in detail, and concluding with examples. Two chapters on the methods of execution of engineering drawings, and the selection and use of drawing instruments, furnish the student with common sense practical hints on subjects which, though secondary, are not unimportant in regard to rendering drawings clear, neat and intelligible.—*Builder.*

THE MECHANIC'S GUIDE: A PRACTICAL HAND BOOK FOR THE USE OF ENGINEERS, MECHANICS, ARTISANS, &c. By W. V. SHELTON. Charles Griffin & Co. Price $3.75.

The compiler of this treatise, who is foreman of the Imperial Ottoman gun factories at Constantinople, states as his object "the gathering into one connected *whole* the principal subjects relating to various branches of the mechanical art, and placing before readers who may not have much leisure for study a concise and simple explanation of general principles, together with illustrations of their adaptation to practical purposes. . . . . The book is the work of a practical mechanic, who may not have the language of a professor at command, but who has tried, to the best of his ability, to supply, honestly and thoroughly, information such as he knows to be greatly needed by intelligent mechanics of the present day." The book is, in fact, intended to be to the working mechanic what Molesworth's and other pocket books are to the engineer and architect. It is a book of reference giving arithmetical formulæ and practical instructions for the carrying out of a great many problems in mechanical work, especially in regard to the setting out and proportioning of parts of machinery. Numerous tables, of the weights and specific gravities of materials, the circumference and areas of circles, &c., are added in an appendix; and the volume appears to be the result of considerable thought and care, as well as practical experience. Short treatises on arithmetic, practical geometry, and mensuration, precede the experimental chapters.

MISCELLANEOUS.

PLUMMET LAMP FOR SURVEYING IN MINES.—An ingenious lamp for the use of mine surveyors has been designed by Mr. Heller (of Heller and Brightly), of Philadelphia, and was described in a paper read before the American Institute of Mining Engineers at the St. Louis meeting. The improved lamp can be used either with or without the safety apparatus, according as fire-damp may or may not be present. The safety apparatus resembles to a certain extent that of the Musseler lamp. It consists of a ring and plate united by four rods. The plate has a cylindrical hole in the middle, and four apertures distributed radially around it. In the centre cylindrical hole is fitted a conical brass chimney, which projects below the plate and is fastened thereto, being kept vertical by four wire braces, or stays, which are soldered to the top of the chimney, and to the outer edge of the plate. The top of the chimney terminates in an in inverted frustum of a cone which is made hollow, and is drilled full of small holes. The inside is lined with one thickness of wire gauze. On the upper part of the cone is screwed a brass cap, composed mainly of a brass ring and wire gauze; the smoke, &c., pass out through the latter. This cap must be cleaned from time to time, depending upon how much the lamp is used, and how much it smokes. It is as well to carry an extra cap in the pocket, which can be put on when the dirty one is taken off. An easy way to clean the cap is to allow a jet of steam to blow through it. The four radial apertures in the plate are also covered by two thicknesses of wire-gauze. Between the top of the plumb bob and the bottom of the plate, and inside of the four vertical wires, is inserted a cylinder of glass. When the safety apparatus is to be used the compensating ring is removed from the ring and placed upon the plate, which has two conical holes corresponding to those in the ring; the ring is unscrewed from the top of the plumb-bob, and another ring is screwed on in its place with the glass cylinder on top of the plum-bob. As the second ring is screwed up the glass cylinder is clamped between the plumb-bob and the plate, making nearly an air-tight joint; the lamp having been lighted before the safety apparatus was screwed on, is now ready for use. The air passes down through the four radical orifices in the plate, which are covered with two thicknesses of wire gauze, is heated by the flame and rises through the chimney passing out through the wire gauze top. The glass is quite thick and well annealed. He has allowed the lamp to burn nearly an hour, until the glass was quite hot, and then thrown cold water upon it without producing any effect whatever on the glass. The wick should not be high, as a very short

one gives light enough and not much smoke. The best kerosene (of as high a test as possible) should be used in the lamp, as the latter gets warm. The top of the wire gauze covering of the chimney becomes more or less clogged with lamp-black, which can be removed from time to time with a fine brush.

DIAMOND ROCK BORING.—A party of gentlemen connected with mining, amongst whom were Messrs. H. Cain, C. E. Bainbridge, J. Walton, T. D. Bolton, T. Rummey, V. Hodgson, F. H. Edwards, T. Kell, and others, met at the Hope Level, Stanhope, on Saturday last, to witness the work which is now being carried on by the Diamond Rock-Boring Company, under the superintendence of their agent, Mr. C. Adkin. Major Beaumont, M. P., the managing director of the company, and inventor of the system, was present, and fully explained the working of the machinery, which consists of a motor, similar in construction to a horizontal steam engine, worked by compressed air, the exhaust serving to ventilate the tunnel. The machine itself consists of a bedplate, on which are fitted two standards; on these are fitted movable saddles for carrying the drills, which can be worked at any angle and in any position, power to drive these being given from the motor by means of a diagonal shaft, driving bevel gearing. The drills consist of a brass quill and nut, mounted in a cast-iron frame, through which passes a hollow screwed drill bar, on the one end of which is fixed the crown, or boring tool, which is simply a small steel tube, set at the end with pieces of carbonate (diamonds in an uncrystallized state.) On the other end is screwed a water union, fixed to a flexible pipe, through which is forced a supply of water when drilling which not only tends to keep the crown cool, but also removes the debris resulting from the borings from the holes. On the nut is placed a friction clutch, so arranged by means of a screw that should the drill come on strata of such a nature that it cannot be bored at the maximum speed, the friction nut slips, and only allows the nut to feed forward the drill bar at the actual speed at which the rock is bored, which was, as we saw on Saturday, in hard limestone at the rate of 4 in. per minute.

The method of working is as follows:—The machine, which is on wheels running on rails laid on the floor of the tunnel, is run to the face; the standards are then tilted forward into position by means of power supplied from the motor, and firmly fixed to the roof by means of screw-jacks at the top of the standards. A set of holes are then put in at various angles and in different positions in the face of the rock from 4 to 5 ft. deep, when the standards are tilted back, and the machine run back to a safe distance, when the holes are blasted, and the debris removed.

The work at Hope Level was, previous to being taken up by the Diamond Rock-Boring Company, being driven by hand labor at a speed of from 1 to 1½ yard per week, while now the rate of progress is from 10 to 12 yards, thus clearly showing that a speed of eight times that of hand-labor can be obtained by the use of this machinery, which where the mineral resources of a place is required to be fully and speedily developed would be a decided advantage, and we doubt if similar results than those above given can be obtained by any other machinery; and in a district like Weardale, where so much mineral is yet undeveloped, we are surprised that the Diamond rock-boring machinery has not been more generally adopted.—*Mining Journal.*

BRICKS AND BRICK-DRYING.—We have received some particulars of a brick-making process at works set up by Mr. Stephens at Kidwelly. Upon entering the works the first thing to be seen is a stone-crushing machine, into which the stones are cast and crushed to pebble size; from thence they are shoveled into a pan, over which two large rollers are worked, which grind the stone to powder. Both the pan and rollers are lined with chilled iron, and so are proof to damage by the hard silica stones from Mynydd-y-garreg. While the grinding process goes on a small white stream of a liquid compound falls into the pan which brings the powdered stone into the substance of mortar; then it is delivered to the moulder, who deposits the composition into a double-mould, places it into a press, which answers the purpose of pressing closely the bricks and of forcing them out of the mould. Then a lad carries them on a sheet of iron and places them in the oven for drying under the new process. This oven is constructed very much on the same principle as a baker's oven, only considerably larger, but in lieu of one floor their are several tiers constructed of upright and horizontal irons about 5in. apart, upon which to lay the bricks, so that the oven can be filled from bottom to top, capable of storing about 10,000. When the oven is full, the iron doors are closed up air tight. Certain flues admit the hot air, and the bricks are said to be thoroughly dried in from three to four hours. Under the old process it would take twelve hours, with the consumption of two tons of coal and the labor of several persons over a large area of ground to dry 5,000 bricks; under the new system 10,000 bricks can be dried in three or four hours with 2 cwt. of coal, with less than half the manual labor, and without the extreme exhaustion caused to the workmen from being hours in the old dry-houses. The originator of the idea is Mr. P. Conniff, an experienced man in the trade.

REFRACTORY CLAYS.—The study of the refractory properties of a clay of given composition is one most important to metallurgical operations. Dr. Carl Bischof has for some time been devoting his attention to the investigation of this subject, with the double object of estimating the refractory properties of a clay of any given composition, and also their respective behavior in the presence of liquefied metal. He has found a wonderful relation almost constant between the chemical composition and the properties of any clay provided that the physical conditions are in all cases the same. The refractory power of clays is determined by the quantity of pure pulverized quartz with which it is necessary

to mix them in order that they should present any considerable resistance at high temperature. Instead of the quartz, a mixture of equal parts of silica and alumina may be used with advantage, in order to obtain even greater precision still in the results. The proportion of this mixture added should be rather greater than that of the quartz. The refractory properties of the clays are represented by reference to a standard clay whose refractory power is taken at 100. This typical fireclay, when a portion of the mixed silica and alumina has been added, and been exposed to a heat sufficient to melt iron, breaks with an earthy fracture, and seizes the tongue when applied, and absorbs an ink-mark traced by a pen on its fracture. This should be the characteristics of all the good refractory clays. To find the respective co-efficients in each case, multiply the reduction or increase in the quantity of mixed silica and alumina added (taking the amount of the typical clay as 1) by 10 and subtract the product from 100, the remainder will give the respective refractory co-efficients of the different clays, that of the type being 100.

The action of liquid cast-iron on the clays has been estimated by mixing four parts of iron with 100 parts of the clay investigated. At the melting heat of wrought iron, the influence of the oxyde of iron has been found nil; the lime, however, and the potassium have produced a vitreous surface. The manganese produces a similar effect, taking place intermediately with the lime and potassium. The chemical analysis and the experiments have clearly shown that the proportions between the alumina and silica, or between the alumina and the cast-iron, vary in the same proportion as the co-efficients of resistance. This rule was subject to a few exceptions, but it was proved that these exceptions were owing to the physical condition of the clay. It is then but necessary to pay attention to dryness or dampness of the clay to obtain accurate preknowledge of results from the chemical composition of the clays.

The above general principles will also apply equally as well to the case of clays subject to the action of glass, of slags, of metals, of metallic oxydes, of bases, and of salts, of cinders, &c. There is a perfectly definite composition to be produced in the typical clay to give the best possible refractory and resisting powers. Here, again, the action of the metal, &c., on the clay is found to be less strong as the co-efficient of the refractory power rises.

Roux and Sarrau have previously shown that two different kinds of explosions can be produced by dynamite, according as the substance is made simply to deflagrate (explosion of the second order), or to detonate by the percussion of fulminate of mercury (explosion of the first order), and that the force of the explosion produced by the same quantity is very different in the two cases. They now find that the majority of explosive substances, gunpowder included, possess the same remarkable property. The reciprocal of the weight (due corrections made) of each substance, which when exploded in one and the other manner sufficed to rend similar cast-iron shells, gave the relative explosive forces. Some results of the experiments are given in the following table, the explosive force of gunpowder igniting in the ordinary manner being taken for unity:

Name of substance.	Explosive force. 2d Order.	1st Order.
Mercury fulminate	—	9.28
Gunpowder	1.00	4.34
Nitroglycerine	4.80	10.13
Poroxyl (gun cotton)	3.00	6.46
Picric acid	2.04	5.50
Potassium picrate	1.82	5.31
Barium picrate	1.71	5.50
Strontium picrate	1.35	4.51
Lead picrate	1.55	5.94

Of the highest practical importance is the discovery of the detonative explosion of gunpowder induced by the detonation of nitroglycerine—itself set off by the fulminate of mercury—for the force of the explosion is more than four-fold greater than that obtained by igniting gunpowder in the ordinary manner. The increased force of gunpowder and gun cotton, when exploded by the agency of detonation, was fully demonstrated by Abel six years ago. The authors observe that the mass of the substance employed for exciting detonation must usually bear a certain proportion to that of the substance to be exploded, but in some cases the action is propagated throughout the latter when once up at any given point.—*Engineering.*

Electric Resistance of Various Metals.—M. Benoit has measured with great precision the electrical resistance of various metals at temperatures from 0° to 860°. He employed both the method of the differential galvanometer and of the Wheatstone's bridge, and for each method has measured several specimens. The mean of these is given in the following table, the second column giving the resistance of a wire, 39.37 inches long and having a cross section of 0.03 inches in ohms, and column three the same quantity in Siemens' units. Column four gives the resistance compared with silver:

Metal.	Ohms.	Siemens.	
Silver, A	.0154	.0161	100
Copper, A	.0171	.0179	90
Silver, A (1)	.0193	.0201	80
Gold, A	.0217	.0227	71
Aluminum, A	.0309	.0324	49.7
Magnesium, H	.0423	.0443	36.4
Zinc, A., at 350°	.0565	.0591	27.5
Zinc, H	.0594	.0621	25.9
Cadmium, H	.0685	.0716	22.5
Brass, A (2)	.0691	.0723	22.3
Steel, A	.1099	.1149	14
Tin	.1161	.1214	13.3
Aluminum bronze, A (3)	.1189	.1243	13
Iron, A	.1216	.1272	12.7
Palladium, A	.1384	.1447	11.1
Platinum, A	.1575	.1647	9.77
Thallium	.1831	.1914	8.41
Lead	.1965	.2075	77.60
German Silver, A (4)	.2654	.2775	5.80
Mercury	.9584	1.0000	1.61

A, annealed; H, hardened; (1) silver .75; (2) copper 64.2, zinc 33.1, lead 0.4, tin, 0.4; (3) copper 90, aluminum 10; (4) copper 60, nickel 25, zinc 25.